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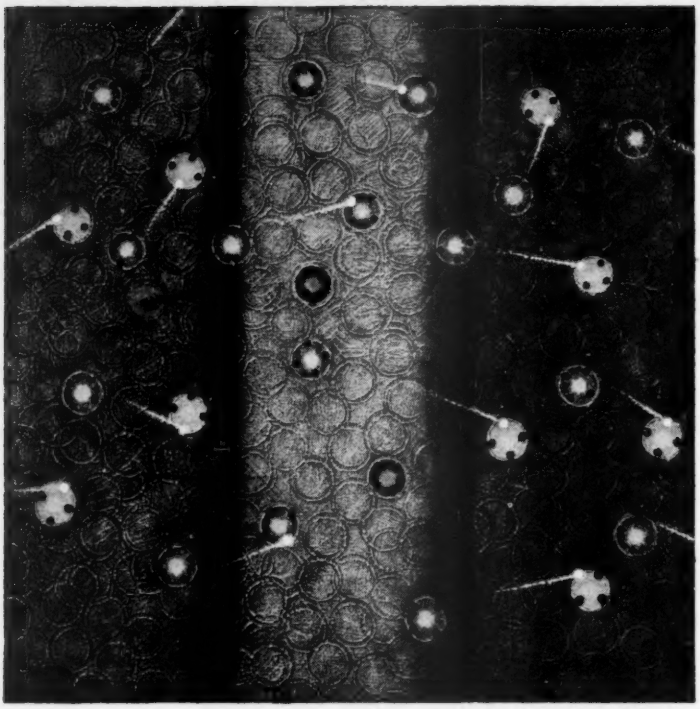

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PHYSIOLOGY AS AN EDUCATION AND AS A CAREER

At the Glasgow Meeting of the Association Section I (Physiology and Biochemistry) and Section L (Education) held a joint meeting with the Physiological Society on the morning of Wednesday, September 3, 1958, on the subject, 'Physiology as an Education and as a Career'. The meeting was held in the Department of Physiology, Glasgow University, with Sir Lindor Brown in the Chair. Seven papers were given and are printed below in the order in which they were read; limitations of space have made some editing necessary. The opening address by Prof. A. V. Hill was read in his absence by Prof. R. W. Gerard of the Department of Physiology, University of Michigan. In the discussion following the papers Professor Gerard described some results from a Survey of Physiology in the United States.[1] He has very kindly summarised these facts and they are included at the end of the papers under the general heading of 'Discussion'.

INTRODUCTION

By Prof. A. V. Hill, C.H., O.B.E., F.R.S.

There is very little new that I could say on 'Physiology as an education and as a career'; but it may be useful to recall a few things I have said before, over the years. In 1920, for example, more than half my life ago, I wrote as follows in *Discovery* (*why* I can't remember, it seems odd for I was busy then trying to relearn the physiology I had forgotten during 4½ years in the Army, but it may be pertinent to the present occasion):

> Hitherto physiology has had two roles, that of elucidating problems provided by the living animal, and that of the hand-maid of medicine attempting to describe the normal healthy functions which medicine has to restore after they have gone wrong. To these two roles is now being added a third—that of a student and investigator of the human or biological factor in the social, economic and industrial system.
>
> In the last resort our political and social system rests upon the mental and bodily characteristics of man, and these depend largely on heredity and environment. The degree to which the behaviour and the bodily, mental and moral "fitness" of men depend upon environment and heredity is not yet known: within limits, however, "fitness" and efficiency depend on adequate training and nutrition, and mental, moral and bodily health on heredity and example as well as on environment. A man's natural powers fit him usually more for one trade than another and it ought to be possible often to state more precisely what that trade should be. Hitherto selection, both in national and in individual life, has been left to nature and man has progressed slowly and unconsciously to an unknown goal. Recently, however, he has taken upon himself so largely to control, by understanding, the forces of nature, that a whole new crop of problems is springing up insistently around him. Large urban populations have arisen as a result of the conquests of hygiene and engineering; plagues have been abolished by the discoveries of medicine; famines have been mitigated by facility of transport, and highly skilled and specialized trades have arisen calling for a new type of average man. Moreover the coming of general education and self-government, together with the growth of class-consciousness and nationalism, have made it necessary to exercise the greatest care in the applications of science to man, the biological unit. These applications however, are bound to come: man, having taken upon himself to prevent plagues and mitigate famines, will have to limit population, to improve industrial efficiency and to eliminate waste. All men get sick and die, so all pass within the purview of medicine. But many men, for the larger part of their lives, are normal healthy individuals; many others

[1] R. W. Gerard (1958): *Mirror to Physiology; a Self Survey of Physiological Science.* American Physiological Society, 9650 Wisconsin Avenue, Washington 5, D.C.

would have been had adequate studies been made of the conditions required to maintain their efficiency. These men require not medicine but physiology, to discover, develop and utilize their normal powers and functions to the full and to see that the lives they lead are of benefit to themselves, their neighbours and their descendants.

Nutrition, hours of work, standards of physical and mental fitness, recreations, physical and vocational training, the effects of various habits, eugenics and what to breed for if we could, these are among the many points at which the economic and social problem touches the biological one. Physiology as yet can give no certain answer to these problems, partly because of their inherent difficulties, but largely because it has tended, with medicine, to regard the human factor from the bedside. It is necessary to found a new profession, Physiology, charged with studying the biological factor in normal human affairs.

How is the profession, as distinguished from the science, of physiology to find its place in our social system? Medicine is maintained by the urgent need felt by the sufferer for relief and help: the normal man equally may need advice and help, but he has not the same potent realization of his need. It is necessary to call in motives more far-seeing than pain or fear. Such a motive may be found in the educated self-interest of a great industrial concern, which has appreciated the value of an impartial study of the human factor in its success: or it may be found in the foresight of government or municipal bodies, or in the generosity of private benefactors, who have realized that the scientific study of normal man in relation to his inheritance and his environment is an essential stone in a rational structure of society. But in whatever way the profession can be created, it has no lack of opportunity for fruitful service.

The most urgent need of physiology is discovery—and this can best be done at universities, with the intellectual stimulus of university life. Progress is within sight in all directions if only workers, equipment, funds and leisure can be made available. But accompanying this discovery there is required, scarcely less insistently, an interpretation of its meaning in terms of human life: and this interpretation can be made only if there be a sufficient body of "professional" interpreters, men well trained, at least as well trained as the medical profession, in science, and given the duty of applying physiological methods and results to industrial, economic and social problems.

So much then for half your title—physiology as a career. As regards its place in education, in 1924, in a lecture to the Pathological Society of Philadelphia, I said:

In many countries the only pathway to physiology is the long and troublesome one which leads a student to a medical qualification. Little wonder that the ablest intellects are often side-tracked into physics, chemistry and biology, which make no such demand. Pasteur was a chemist, Bayliss was catholic in his science but had no medical degree, Barcroft, Langley, Hardy, Mines and Lucas (to name a few from the school of physiology at Cambridge [1]) came to physiology through gates other than those of medicine. Physiology then must have its roots in science, must attract students who have no intention of following medicine as a profession. Here it labours under one great difficulty, it is an intermediate science, physics, chemistry and biology must precede it and quite naturally the ablest students tend to get filtered off and retained by the simpler and more elementary sciences. But the difficulty is not insuperable. At present there is a tendency to specialise too young. It should be impossible for students of science absolutely to neglect the study of physics, chemistry and mathematics as they sometimes do; it should be equally impossible to neglect the study of at least one biological subject as they usually do. If only that principle were conceded, we physiologists would get a fairer share of the best brains.

In another lecture, in 1930, referring to general ignorance of biology, I said:

Even among scientific men, many are quite innocent of any acquaintance with that major half of natural knowledge which comes in the category of biology. What would they say of a biological colleague who knew nothing of elementary algebra, who had never learnt to use a balance, who confused momentum and energy? So great indeed is their innocence that many of them suppose the problems of biology to be quite simple, only rather messy and inexact. How many historians, scholars, lawyers, economists, philosophers, have any biological knowledge? Yet what would they think of a biologist who was totally ignorant of history and literature, and could not read any foreign language?

[1] A lot more could be named by 1958.

My friend A. D. Ritchie, a philosopher who is *not* ignorant of biology, reading this, told me that two and a half centuries earlier, Robert Boyle had insisted that it was '*highly dishonourable for a reasonable soul to live in so divinely built a mansion as the body she resides in altogether unacquainted with the exquisite structure of it*'. Most things have been said before—and usually better! Though I still like one or two of the phrases in that lecture:

> Latin is said to be essential to the proper understanding of literature: biology only to the proper understanding of man. . . . Are the implications of evolution less significant than those of the binomial theorem or of Latin grammar?

In another talk, in 1932, I had some more provocative remarks to make:

> It is apt to be thought that biology is a fit refuge for those whose brains are not good enough to do physics and chemistry.[1] It is true that to do biology badly is easier than to do physics badly—I am not sure that it is easier than to do chemistry badly, many people can do that. But to do biology well is harder than to do physics and chemistry well: the subject matter is more complicated, the material less manageable, the conditions are harder to control. Those who have worked on both sides of the boundary between physics and biology know well on which side the more difficult problems lie.

But the battle for the intellectual status of biology, and of physiology in particular, among the sciences, as of the status of science in a humane and civilised education, is really being slowly won; and the main purpose of this symposium is to discuss ways and means. One thing we can be certain of is that most people, even (or particularly) the youngest, are interested in the machinery of their bodies and in the ways in which living things work, plants and animals alike. The impulse to try to understand these things is usually strong enough to make the task of a teacher of physiology an easy and attractive one. I was fortunate myself to learn mathematics from an excellent teacher and a very wise man; instead of putting us first through the drudgery of mastering, for example, the classical proofs of algebraic theorems, he would introduce us to some of the many interesting ways in which these theorems could be applied. Then we were the more ready to try to understand the proofs. So in physiology: there are innumerable aspects of physiology about which people, and particularly boys and girls, can get excited. Thirty years ago I gave the Christmas Lectures for children at the Royal Institution and after consultation with my own children decided to talk about 'Living Machinery'. To quote from the Introduction to my book.

> The best suggestion made came from J. (aged 8), who proposed that I should make experiments upon her in front of the audience, a proposal seized on at once by the rest. Fearful experiments I would make: P.'s heart (aged 12) should be shown beating, her lungs should be measured up, perhaps even her bones should be shown, her emotions should be exposed on a screen: D. (aged 11) should be given electric shocks till sparks came out of his hands, he should climb to the top of Monte Rosa and pedal a bicycle there (a strange place to find a bicycle) till exhausted, then he should be given oxygen and feel fresh again at once. J. should have the movements of her stomach (there is no decency about young ladies these days) shown to the audience, while the noises made by M.'s heart (aged 7) should be made to sound like a gun round the lecture hall, he should breathe strange gases out of a bag, and he would not be content till I had promised that he also should have electric shocks. Alas, not all of this could be done, but some of it could.

The result was quite satisfactory, and illustrated in an elementary way how people, and particularly children, can have their interest aroused in human physiology. The same educational technique can be applied in many other ways and in other aspects of biology: and once the interest is present the desire will follow to learn and understand more. Thus, although physiology as a scientific study cannot be

[1] Twenty-four years later, in a lecture 'Why Biophysics?', in an attempt to define biophysics, I remarked, 'One thing it clearly is not is a second-rate branch of physics, a haven of refuge for indifferent "physicists" '.

properly undertaken without a basic knowledge of other sciences, it *is* possible to introduce physiology at a very early stage in education—as it is with natural history. To do so will serve to attract recruits later on, and with the rest will help to meet Robert Boyle's insistence of two and three quarter centuries ago.

PHYSIOLOGY IN UNIVERSITIES

By Dr. J. A. B. Gray

Physiology emerged as an independent university subject about a century ago. Since then it has developed mainly for the purpose of training medical students. Medicine no longer holds a monopoly of physiology; but I think it would be true to say that, outside the universities, physiology is regarded as being a subject taught only to medical students and practised only by medical men. While a close relationship between medicine and physiology is of the greatest importance, physiology, as an independent science, has other equally important roles to perform; in other professions, in the control of animal population and as an important educational factor in the development of science as a whole.

In order to elaborate on this theme it is necessary to indicate by examples the scope of modern physiology. Physiology is the science concerned with the mechanism by which animals live; it considers the physical and chemical processes that constitute that mechanism and the relation of these processes to the environment of the animal. Thus work is being done on the physical and physico-chemical properties of cells, and the chemical reactions that provide the energy necessary for cellular activity. Of the studies of whole organs I would like to pick out one for special mention—the nervous system. This is a field in which research has increased greatly in recent years, particularly on the basic principles on which the activity of the nervous system depends. Applied human physiology is flourishing and physiologists now assist the Services in such fields as diving and flying, and accompany expeditions to Everest and the South Pole in order to carry out their investigations. One final example: comparative physiology. In this field studies are made of the different mechanisms which have been developed by different groups of animals in their attempt to adapt to different environments; these studies not only increase our knowledge of these mechanisms, but also attempt to answer the question 'Why?' in biological terms.

To return now to the role of physiology in the teaching of science in universities. The teaching of physiology in the faculty of science has increased greatly in recent years; for example, at University College, London, last session there were more than eighty students who were registered for degrees in the faculty of science and who were taking courses in the department of physiology. More than half of these were registered for degrees in other sciences and were taking physiology as an ancillary subject for either one or two years of their university course. In this connection it is very interesting to note that the University Colleges of Ibadan in Nigeria and of Ghana, both of which take degrees of the University of London, have recently been teaching physiology as an ancillary subject to their students taking a special degree in chemistry. As the University College of Ghana has no department of physiology this teaching has been done by the Zoology department. There is evidence that a similar need is felt in other universities and colleges in which there are no departments of physiology. For example, the University of Exeter has run an excellent vacation course in physiology for teachers and the University of Southampton advertises post-graduate training in physiology. At Imperial College also there is no department of physiology, but one of their professors of physics has made notable contributions to the physiological literature.

Why has this need for physiology as a part of the general teaching of science arisen and why do I believe it is going to continue to grow? Physiology has, I believe, a unique position amongst university subjects. It is a biological science, but it is also an experimental laboratory science which can provide for the morphologist an introduction to experimentation. In its application of physics and chemistry to biological problems physical scientists may become acquainted with biological thinking. Such an introduction to biology can I believe be of great value to physicists, chemists and engineers. Apart from the obvious educational advantage of a broader view of science, they will find that many of the practical problems they wish to solve have been solved, not by biologists, but by biology. It may be that today the biologist is so far behind his physical colleague that the physicist is unlikely, as he did in the past, to gain new ideas from the biologist; as, for example, when the physiologist Galvani opened the way to the study of current electricity and when another physiologist, Poiseuille, opened up the field of hydrodynamics because of his interest in blood flow. Maybe such events will not be repeated, but I am inclined to believe that they may. The body's own measuring instruments are far smaller and far more sensitive than anything devised by man; the conversion of chemical energy into useful work is performed efficiently by animal muscles over a wide range of conditions; and the nervous systems of even quite simple creatures provide examples of control mechanisms that should be the envy of our modern engineers. Perhaps we will find in the long run that a study of the nervous system is more help to the engineer, than are the engineer's machines to telling us how the nervous system works. That physiology deals with man as well as with other species can, in itself, be a humanising influence in the faculty of science, but there is also a practical reason for giving physical scientists and engineers some knowledge of human physiology. Many graduates in these subjects now help to design machines, processes, etc., the uses of which are limited by the capabilities of the human operator. There should be physiological advice available to the designers and they should be in a position to understand the language of the physiologist, just as the physiologist must be able to use the language of the physicist, the chemist and the engineer.

Physiology is closely related to psychology. This relationship has been strengthened in recent years by the increasingly experimental trend in academic psychology. In practice this has been reflected in my college by a large swing, on the part of psychology students, from taking arts degrees to taking science degrees and also by a large increase in the number taking physiology as an ancillary subject. This link between physiology and psychology is significant in view of the growing importance of normal psychology to the social problems of modern society and because the physiology of the nervous system and the mechanisms underlying sensation are a basis not only of psychology, but of philosophy and the meaning of knowledge itself.

I have given a number of reasons why I think physiology should be taught to many undergraduates of the faculty of science and I have also given some factual evidence to suggest that there is a growing demand for such teaching. I believe that physiologists should not only provide such teaching when it is required, but also foresee the needs of the community and be prepared to give a lead when needed. Courses must be designed to be of value to the recipients. What is required is not always what a medical student needs; for example, medical courses are rightly strong on the circulation and respiration of the mammal, but these topics are not always the most important for scientists.

I have spoken mostly about the educational role of physiology in a university. Careers in a university arise as a corollary, because people are needed to teach these courses and to carry on the pure research on which any science must be based. I have spoken of the value of physiology to other sciences, but my remarks

must have made clear the importance to physiology of physics, chemistry, mathematics, psychology and zoology. A university where all these subjects are grouped together is the ideal place for fundamental research to be done.

I would like in conclusion to return to a point I made earlier. Ancillary classes in physiology in some universities are now large. Yet there are a considerable number of universities and university colleges in this country which have faculties of science but have no departments of physiology. In the present planned expansion in the sciences and in technology, I believe it is most important that physiology should play its part both in fundamental scientific research and also as an applied science. The achievement of such an aim would ultimately entail the development of physiology in universities which have faculties of science but not faculties of medicine.

PHYSIOLOGY IN SCHOOLS

By H. P. Ramage

In the school stage of education we need to remember that Physiology is a branch of biology and that biology is a part of science. Furthermore, University courses in physiology can only benefit a very small proportion of the country's whole population, simply because most people do not go to a university. However, on the basis of one vote for each adult, those who never reach a university are, from sheer weight of numbers, more powerful politically than those who do. It is therefore the schools who have to provide the balanced curriculum needed by the general population of an educated democracy.

The question of physiology in schools goes further back than many realise. As long ago as 1864 the first of several Royal Commissions on Education, that which enquired into the administration of nine great public schools, felt obliged to condemn the almost complete exclusion of Natural Science from the education of the upper classes in England as 'a plain defect and a great practical evil'. The Commissioners urged that Natural Science should be taught where practicable and should include two main branches, one comprising Physics and Chemistry and the other Comparative Physiology and Natural History. That would have been a broad and balanced course. Further, such a course was to be taught for its general educational value, not just to provide elementary vocational training for future professional scientists and technologists. Enough is being said by others in this symposium about ways in which physiology is needed in the vocational training of those entering such careers as medicine, agriculture and industry. I want rather to discuss the potential value of physiology, as a part of biological science, in the general education which is the primary objective for the school stage of education.

Unfortunately, the hopes of the Royal Commission of 1864 were not fulfilled. There were some honourable exceptions, but, in general, the science courses developed in schools were of too narrow a kind, mainly because of vocational demands which overrode the claims of general education. For the most part, boys studied physics and chemistry only, while girls studied botany, presumably considered more ladylike. If any physiology was taught it was of the narrow physiology and hygiene kind and even that was discreetly expurgated. Other reasons for the much greater attention given to physics and chemistry by boys were that those sciences, dealing with simpler subject matter, had developed faster and further than biology and they also had greater applicability to the great industrial developments then taking place. However, the narrow school curriculum as regards science meant that pupils were getting their conception of science from a study of strictly limited parts of it. Even the physics often consisted of only sections of that subject,

such as heat and light *or* electricity and magnetism *or* mechanics and hydrostatics, while the approach to science merely through chemistry led someone educated in that way to say, 'Ah, stinks, that's what we used to call it at school', when I was introduced to him as being a science teacher.

Worse still, physics and chemistry came to be regarded as proper, basic sciences which would in time satisfactorily explain everything. Physiology was only acceptable as science in so far as it was thoroughly materialistic, which led one cynic to remark that 'the man who calls his brother an ingenious mechanism is either a fool or a physiologist'!

Along with this over-emphasis on the so-called exact sciences, as though that made them qualitatively superior as well, came excessive reverence for mere numerical exactness and too rigid a conception of scientific method. Science is methodical, but there is no such rigid thing as what is called 'The Scientific Method'.

The result of the foregoing unfortunate trends was an accentuation of the already restricted use of the term *science* in Britain as contrasted with its wider and better connotation on the Continent. No wonder the breach in the body of knowledge, which began to appear in the eighteenth century, has become a gaping wound, so that we have the conflicting groups, respectively calling themselves scientists and humanists, though neither with much justification. Enormous harm has resulted and one of our most urgent tasks in general education is to try to heal that gaping wound.

It is true that more boys do biology than formerly, that girls have largely changed from botany to general biology and that the old physiology and hygiene courses have broadened into what is increasingly being called human biology, but much narrowness still exists, with boys still mostly doing physics and chemistry only and girls mostly biology. This is reflected in staff qualifications, where the vast majority of men science teachers are physicists or chemists or both, whereas nearly half the women science teachers are mainly qualified in biology.

Something must really be done to provide fuller, better-balanced science courses in schools and this is where I come to the '*Science and Education*' Policy Statement published during the past year by the General Committee of the Science Masters' Association and applicable particularly to Grammar Schools. This recommends that all pupils below the sixth form should have a balanced course of science including physics, chemistry and biology, with also some astronomy and geology, that all sixth-form pupils should have a cultural course of science embracing the history and philosophy of science, its present-day social and technological consequences and its future possibilities, and, thirdly, that specialist syllabuses in the sixth form should be reduced in factual content to allow sufficient time for that cultural course.

We are now working on detailed syllabuses and panels have been set up to deal with particular portions. The biology panel has nearly finished its task and, in conclusion, I want to quote parts of a paragraph from the introduction to our syllabuses.

> Educational subjects which can act as links between a number of other subjects are in a key position and the biological part of science can have a special significance in this respect. Biology (with physiology within it) reaches out in one other direction, through biochemistry and biophysics, towards the physical sciences and in the direction, through psychology and the social sciences, towards history and the arts. Through Biology, too, we can draw attention to the often forgotten subjectivity involved in the physical sciences.

There are many other ways in which school biology can be of value, but I suggest that, in our present situation, the linking function I have indicated is the most important.

PHYSIOLOGY IN MEDICAL RESEARCH

By Dr. W. A. Cobb

It was with some reluctance that I agreed to take part in this discussion because my contact with its theme is on a very narrow front. However, I was persuaded that the history of electroencephalography, a sub-speciality within a special branch of physiology, might be of interest as an example of what may happen when a new biological discovery has direct application to medicine.

The discovery that potential changes generated by the brain could be recorded from the overlying scalp of man was made, contrary to all probability at that time, by a psychiatrist, Hans Berger of Jena, whose first paper was published in 1929. This work was largely ignored, despite seven further papers, until it was given respectability by physiologists five years later. Indeed, it could not have been made more respectable, for the physiologists were Adrian and Matthews of Cambridge.

Curiously, it was a non-medical physiologist, Grey Walter, who first showed in 1936 that electroencephalography, which was Berger's own cumbersome word for the brain-waves which he had discovered, could be of medical value; by its means Walter located the position, with reference to the scalp, of brain tumours. He was quickly followed by others, mainly in the States, who demonstrated extraordinary wave patterns from the brains of epileptics. By the beginning of the war the clinical EEG was fairly launched on its career in the U.S.A., in France, Germany and England. A parallel development was the interest of psychologists in what seemed to be a promising tool for investigating conditioning, reaction times, and the like.

Expansion in the use of the EEG since the war has been fast and general in the Western world. While a few laboratories are devoted to psychological or physiological research, even these are often attached to hospitals as sources of human material. The great majority of the rest have the primary function of serving the diagnostic needs of physicians. The senior personnel of such departments come from a number of disciplines, from neurology, psychiatry, a few physiologists and psychologists, even an occasional physicist, and sometimes, such as myself, by chance from no particular discipline at all.

Whatever our training, the initial fascination of a virgin field, the correlation of patterns and wave-forms with cerebral diseases, could not remain a sufficient interest in itself, and, if diagnosis is still the ostensible primary function of most laboratories, their main hope is to uncover the physiological bases of these same patterns. We have had to learn the cerebral physiology which we had forgotten or never knew and, because the subject is so complex, in most cases to build up teams, usually of neurologist or psychiatrist, physiologist and physicist, to deal with both the clinical commitments and the research projects to which they give rise.

In most branches of physiology an observation made on a well-chosen laboratory animal can be transferred to man with little fear of serious error; in neurophysiology this is probably true also at the cellular level; it is certainly far from being true at the level of the higher cerebral functions and here the only proper study is of man. It is for this reason that, if the clinician has need of the physiologist, equally the physiologist should recognise his need of the physician and the case material to which he can give access.

In the last fifteen years there has in fact been a most fertile exchange of information between the clinical neurophysiologist, the EEG man, on the one hand and the purely experimental neurophysiologist on the other. The matter cannot yet be seen in true perspective, but it seems that the advance in knowledge of brain mechanisms has been greater and faster than ever before. It is the more regrettable therefore to have to acknowledge that the contribution from this country has been relatively small.

It seems to me that there are three reasons for this, which certainly do not include any lack of potentially available skill. In the first place the great school of British neurologists, from Hughlings Jackson to Gordon Holmes, derived so much from purely bedside observation that a firm tradition has been built up and there is little encouragement to devote a part of the training years to experimental physiology, as there is in some of the countries I have mentioned, nor is first-hand experience of applied electrophysiological methods and results usual or expected of the trainee neurologist.

Secondly, the structure of the medical services is not such as to offer any striking financial reward to the specialist in applied electrophysiology, as there may be elsewhere, nor are there anything like enough higher appointments to tempt first-class young doctors to specialise in this field.

Thirdly, there are few physiologists in this country, medically qualified or not, who concern themselves with cerebral function. It is plausibly suggested that a reason for this is the very brilliance of the schools of axonal, spinal and neuromuscular physiology which draw to themselves a disproportionate number of the best brains. It may be relevant, too, that the restrictive effects of the anti-vivisection laws, which have no exact counterpart in America and France, for example, are maximal when cerebral processes in the conscious animal are to be studied. The far greater activity in the field of experimental cerebral physiology in America has had the unfortunate effect of drawing away more than one worker who might have remained to stimulate research in this country.

To return to the subject of neurology. As I have already pointed out there is much neurological research which, by its nature, can be performed on man; some of it, in addition to its intrinsic interest, has more or less direct bearing on the well-being of man in general or a patient in particular, merits to which even the most dedicated backroom boy cannot be wholly indifferent. It must be admitted that man as an experimental animal presents limitations, often of a most frustrating kind, but generally he is very co-operative; you may recall the remark of Sherrington to Penfield: 'It must be great fun to put a question to "the preparation" and have it answer.'

I have spoken largely of the very narrow specialty of electroencephalography; in fact, most of the better EEG laboratories call themselves by some other title such as that of 'Applied Electro-Physiology', and widen their sphere of interest to include investigation, predominantly electrical, of the whole of the nervous system and the muscles as well. Thus in one laboratory with very limited resources, particularly of staff, there has been experimental work in the last few years on peripheral nerve conduction, blocking of spinal roots, various reflex problems, the retinogram and the behaviour of partially isolated cortical slabs.

While in all countries there are EEG departments, which find themselves unable to get beyond the routine commitments of a clinical service, the proportion which does so escape is a measure of the local vitality of cerebral neurophysiology. In America, and France also, possibly in other countries of which I have no first-hand knowledge, the number of laboratories in which the cross-fertilisation of clinical experience and human or animal experiment is occurring is high. Striking evidence of this occurs at scientific meetings at which neurology and neurosurgery find common ground with physiology in discussing the electrical activity of the brain. Here things are quite otherwise and, far from being able to discuss their problems on an equal footing, these specialties seem loth even to acknowledge in public the legitimacy of their common offspring, electroencephalography.

And what of the advances which have been made in the understanding of cerebral mechanisms? Few of them appear as yet to be complete; rather, it seems that barriers have been broken down, leaving the roads more open for the future. By way of example, the necessary foundations, the natural history, of epilepsy had been

firmly laid by the time the EEG came to provide an entirely new approach and to raise more questions than it answered. The experimental neurophysiologist has been provided with a new set of problems which are solved in terms of both the clinical and electrical observations; in their turn the answers to the riddles of epilepsy throw light, as they have always done, on normal cerebral function.

Again, modern knowledge of the causes which underlie the balance of consciousness and unconsciousness is due largely to the animal experimental work of Bremer, Moruzzi, Magoun, Jasper, and their schools, but the crude differences which we can recognise in the state of awareness in the cat are far removed from the subtle disturbances which occur in man. Recent work, mainly in France, shows that the human EEG can be an excellent indicator of the state of vigilance and any theory of consciousness must account for the EEG variation which accompanies its partial or complete loss.

I have given reasons why I think that a department of applied electrophysiology can be a particularly fruitful meeting ground of the pure scientist and the experimentally minded clinician, and there are many examples elsewhere to show that this is so. It would be untrue to suggest that there is a significant number of unfilled vacancies for trained neurophysiologists in such departments in this country; probably there are none at all. But I have no doubt that the jobs would be created if there were men of quality who wished to fill them. It is indeed a necessary task for the future to show our physiological colleagues the importance and excitement of the field which they have left so largely neglected.

CAREERS FOR THE PHYSIOLOGIST IN AGRICULTURAL RESEARCH

By Dr. D. P. Cuthbertson, C.B.E.

Excluding the Universities, the Agricultural Research Council assess that there are some fifty physiologists in state-aided research institutes concerned with animals. All told, there are probably some eighty to ninety physiologists in the country, directly interested in animals of agricultural importance. Lines of research are listed in the Agricultural Research Council's index of research, including some sixteen pages of detailed information about the topics being pursued at places like:

1. The Institute for Animal Physiology at Babraham, near Cambridge.
2. The National Institute for Research in Dairying, Shinfield.
3. The Hannah Dairy Research Institute, near Ayr.
4. The Poultry Research Centre, Edinburgh.
5. The Rowett Research Institute,

and also elsewhere in various University Departments and Colleges.

In a paper to the Jubilee Proceedings of 'The Annals of Applied Biology', Professor Wigglesworth, Quick Professor of Biology at Cambridge, who directs the Agricultural Research Unit of Insect Physiology, pointed out that during the Second World War many biologists who had hitherto been concerned only in carrying out investigations in pure science, were given the opportunity of directing their talents to the solution of immediately practical problems. Many turned to this new field with considerable self-confidence well knowing that applied science was just like pure science, only rather easier. Wigglesworth goes on to describe how for many this was a humbling experience. In the pure science to which they were accustomed, if they were unable to solve problem A they could turn to problem B, and while studying this with perhaps some small prospect of success they might suddenly come across a solution of problem C. But now they must find a solution to problem

A, and A alone, and there was no escape. Unfortunately there has been a tendency to consider that applied physiology is 'physiology for the less intelligent'. It is certainly not so, and it requires a totally different attitude of mind.

Insect Physiology

During the War the scientific study of insecticides was impeded by lack of knowledge of insect physiology. For example, how can the insect take up oxygen and yet keep its loss of water by transpiration down to the minimum—a mechanism which has made insects such successful competitors with man for the domination of the dry land? Investigations revolutionised our knowledge of the histology and chemistry of the cuticle and uncovered an elaborate respiratory system in the eggs of many insects whereby oxygen diffuses in the gaseous phase beneath the shell (a process enormously more rapid than diffusion through yolk).

Problems of resistance to insecticides are also of fundamental interest, for the light they throw on insect physiology. Insects resistant to DDT have developed enzyme systems by which it is broken down to non-toxic products. In turn, it is possible to introduce substances which sufficiently resemble DDT as to block the DDT-dehydrochlorinase enzyme and thus act as synergists serving to increase the susceptibility of those insects which have become resistant—although they have no such action in ordinary susceptible insects.

Sheep Physiology

The highly evolved wool sheep is an outstanding example of artificial selection. Its fleece, unlike the pelage of its primitive ancestors which is shed periodically, grows continuously, and would become an intolerable burden if left unshorn. Its integument is densely populated with follicles that produce wool at a rate grossly in excess of the animal's needs for heat conservation and which imposes nutritional demands that are in many ways unique. The histogenesis, histology of the wool follicles and the biochemistry of fibre growth are rewarding fields as is the influence of nutrition, particularly the specific effects of nutritional deficiencies on wool growth, e.g. copper in relation to character of fleece and its crimp.

The discovery of variants of adult haemoglobin in man has been followed by those of haemoglobin variants in animals. Sheep possess two types of haemoglobin, the incidence varying from breed to breed. One sheep haemoglobin (B) predominates in lowland breeds and the other (A) in mountain and hill breeds. The haemoglobin with the greater affinity for oxygen is the A type that is in the mountain breed. When individual sheep of the heterozygous AB type are made anaemic a change can be seen in the proportion of the two haemoglobins. Production of B is favoured at the expense of A, facilitating the transfer of oxygen from the blood to the tissues. Haemoglobin A is more useful when there is a need to improve the acquisition of oxygen from the air.

Sheep may also be classified quite independently into two types, HK and LK, according to whether the concentration of potassium in their whole blood is relatively high (HK) or low (LK). Both sets of characters are determined genetically.

Environmental Physiology

The salient difference between cattle and man in respect of environmental physiology is that man is a sweating animal and cattle are principally panting mammalia. With cattle the emphasis in environmental physiology is ultimately still on the 'comfort' zones but with reference all the time to productivity, and the physiological measurements are largely those of respiratory physiology. Consequently, the whole technique of experimentation is very different. Many techniques which are difficult to use with small animals and impossible with man can be used with cattle, e.g. it is a matter of comparative ease, as Dr. Findlay of the Hannah Dairy Research

Institute has shown, to have indwelling thermocouples in the carotid and pulmonary arteries in the surviving animal and thus to study under climatic stress the correlation between rectal and true deep body temperature; the mechanism of the initiation of panting; the cooling effect of convection in the lungs and to get a very close idea of the temperature of the blood near the hypothalmic temperature regulating centres.

To any physiologist who delights in new techniques, few fields are less exploited than this one. The sweat glands of cattle are quite different from those of man, both in their structure and in their functional ability and in their response to pharmacological substances. There is a wide field of largely unexplored biophysics waiting to be done on such factors as the effect of light and heat on coat quality; on the insulating value of the coat; on the nature of water transudation through the skin and on blood-flow in haired mammalia.

Acclimatisation in man is a well-studied subject but one about which little is known in cattle. Completely different criteria to measure acclimatisation in cattle have yet to be devised and such criteria must be related to productivity.

Ruminant Digestion

This is a wonderful field for the co-operation of physiologist, biochemist and microbiologist.

The outstanding difference between the ruminant and other herbivores is that the former pre-ferment their food through microbial action in the first two compartments of the complex stomach—the reticulo-rumen sac—and absorb considerable amounts of the products. The reticulo-rumen sac thus forms a fermentation vat in which bacteria and protozoa are established under highly reducing conditions when the animal is quite young. The reticulo-rumen sac is never empty and all the factors affecting it make for a steady state. The activity of the salivary glands not only serves to moisten and lubricate food as it is chewed and swallowed, but also supplies most of the water and alkali that keep the rumen contents fluid and buffered. The process of rumination involving re-mastication, the absorption of the products of microbial activity and the interchange of ions between rumen contents and blood—in the case of sodium against the concentration gradient—all contribute to the continuity of microbial action during the whole life of the animal and produce conditions which one can hardly hope to reproduce *in vitro*. Nevertheless, by observation of the micro-organisms—the bacteria and the protozoa—at the site of their attack on food particles, and in isolation in culture, and by a study of the soluble products of fermentation and their absorption much information about this microcosm has been obtained.

In the rumen fermentation of cellulose thorough microbial activity takes place, otherwise it would be unavailable through lack of a cellulose in the digestive glands. Dietary protein is subjected to fairly vigorous hydrolytic conditions and the low-molecular hydrolytic products including amino-acids, are then assimilated by micro-organisms and incorporated into their body protein, or, if amino-acids accumulate in excessive amounts, the micro-organisms acquire the power to ferment them deaminatively to ammonia, short-chain fatty acids and carbon dioxide. Urea and ammonia can be utilised by the micro-organisms for elaboration of their own body proteins. These are of good biological value and are presumed to be digested and absorbed in a similar fashion to that taking place in the non-ruminant. Fats are hydrolysed by microbial action and the glycerol produced may in part be converted to propionic acids. The unsaturated fatty acids tend to undergo hydrogenation.

The products of carbohydrate fermentation, short-chain fatty acids, together with those derived from protein are absorbed from the rumen into the blood stream. It has been calculated that for hay-fed sheep about 70 per cent of the total

energy requirements are provided by these fatty acids. These short-chain acids are quantitatively much more important than glucose as a source of energy.

While the microbiological synthesis of fat-soluble vitamins (other than vitamin K) has not yet been unequivocally demonstrated the metabolic needs for the various B vitamins are met by synthesis in the reticulo-rumen through microbial activity. The synthesis of vitamin B_{12} is conditioned by the cobalt content of the feed. Ascorbic acid appears to be synthesised in the ruminant's own tissues. Thus microbial digestion confers important benefits upon the ruminant. Energy from fibrous foods, which could not otherwise be obtained, is more efficiently garnered and the available energy is yielded over a longer period of time so that the animal is more resistant to periods of starvation; the uptake of nitrogen is controlled and that from non-protein sources utilised more efficiently via microbial proteins; and considerable synthesis of vitamins takes place.

Interest of Rumen Absorption Outwith Ruminant Physiology

The advantage of rumen absorption studies lies in the simplicity of the structure studied. The diffusion barrier presented by rumen epithelium is much simpler than most others studied, as it consists of parallel sheets of cells, each of which represents a different stage in the existence of one type of cell. No secretory cells are present. The large size of the rumen and the ease of isolating it from the rest of the gut make this membrane ideal for testing absorption uncomplicated by the formation of secretions.

Sodium movement through this epithelium against its electrochemical gradient into the plasma is of interest as it represents a process which goes on all over the body and has been tentatively suggested as the prime agent for moving water, salt and metabolites against concentration gradients. The possibility, at present being studied, that rumen epithelium isolated from its blood supply will continue this activity when *in vitro* may give an opportunity of investigating its mechanism, and response to hormonal control in a relatively simple and accessible tissue.

PHYSIOLOGY IN INDUSTRY

By Dr. J. Yule Bogue

Before discussing physiology in industry, it is worth comparing academic and industrial conditions in relation to routine activities. In the university research is combined with the requirement to teach, and the substance and quality of the teaching is only as effective as the research that backs it. In industry, on the other hand, the teaching requirement is replaced by the responsibility for routine screening, testing or assay. Here again, the responsibilities are only as good as the research behind them.

Industrial routines are developed as the result of research. They may be partly or wholly run by technicians, but the responsibility for them still rests with the research worker. Industrial research has the practical objective to produce a definite industrially useful product or result by scientific methods. The fields include both relatively short-term (3 to 5 years) and long-term projects, such as cancer and virus work. Our research is in fact an investment for the future.

We can broadly divide the outlets for physiology into two main groups.

(1) Where physiology is concerned with the environmental conditions when working in industry and also with the conditions that may result from utilising or working with the products of industry.
(2) Where physiology constitutes part of the know-how or background upon which new products or results are produced.

Let us consider the first group. Physiologists, preferably with a medical degree and an interest in human physiology, have an excellent outlet in studies concerning the limitations of human endurance. Modern industrial developments require personnel to work in a variety of environments not normally encountered in everyday life. For this reason, larger concerns, particularly in the chemical industry, have set up well-equipped laboratories to study their peculiar problems. These investigations include toxic hazards, abnormal environmental temperatures and the most effective design and layout of plants and machines. One well-known industrial institute of this type is equipped with high and low temperature rooms, a treadmill, electrophysiological measuring equipment such as E.C.G., ballistocardiograph, E.E.G., gas chambers, etc., in addition to the conventional facilities associated with their chemical, physiological, biochemical and pathological laboratories.

Time does not permit enumeration of the many problems at present under investigation. Several examples, however, may be of interest to indicate the scope of work being done. The design and development of pressure ventilated protective suits to enable operatives to work in dangerous atmospheres has involved studies in the physiology of sweating, limitations of movement, visual fields and performance. Many potentially hazardous products are produced either as intermediates or resultants of chemical reactions in the manufacture of an innocuous product or deliberately with a view to utilising their toxic properties as insecticides, fungicides, weed killers, rat poisons and the like. It is essential to know the mode of action of these substances on living things, from insects to the experimental animal and, finally, man. Methods must be developed for the early recognition of the toxic effects of these substances in man. In the case of organo-phosphorous compounds, for example, signs of neurotoxicity and esterase concentrations are of vital importance.

The mode of action of carcinogenic and radiomimetic agents on physiological mechanisms is another important field; and the use of radioactive isotopes in research laboratories and factories has brought the study of the physiological effects of radiation currently much more to the fore.

Then there are the obvious problems associated with aviation: high speed aircraft, jet noise, acceleration, instrumentation, servo-mechanisms, vision and equilibrium. In most countries these problems tend to be tackled in government or military laboratories such as those at Farnborough and Wright Field, but some of the work is done in the laboratories of the contributing industries. Acoustic engineering, stereophonics and hearing aids also benefit by a physiological contribution.

Work Study. Here the physiologist is not so much concerned with the limitations of human endurance as with industrial fatigue of the type occasioned by close inspection work or certain kinds of muscle utilisation. Such studies are relevant in determining rest allowances and in the grouping of machine and instrument controls.

The second group, where physiology constitutes an essential link in the industrial research effort, is perhaps best exemplified by the pharmaceutical industry. Pharmacology in industry leans more towards physiology than classical pharmacology. A medical qualification is not essential but most laboratories like to have one or two medically qualified physiologists in the group.

As a rule, before exploratory chemical synthesis can begin, a basic study of the pathology of the disease process and its experimental production must be undertaken. This will include literature surveys and experiments designed to fill the lacunae in our knowledge. It may include insect or micro-organismal physiology in infective diseases; or in the case of diseases of organ dysfunction, physiology of the organ or system involved. At this stage, the team will co-ordinate the efforts of physiologists, biochemists, experimental pathologists and pharmacologists. It is difficult and unrealistic to draw sharp distinctions between each of these

disciplines. An experienced general physiologist may well find himself at different times acting in any of these activities according to his background.

When the stage of speculative chemical synthesis is introduced, there is, in addition to the routine screening, the interest aroused by the turning up of peculiar biological activity or even toxic manifestation which may constitute a useful tool in elucidating the mechanisms of some living process.

We sometimes hear that the scientist in industry works under pressure. This is not true. The only pressure comes from his colleagues in cognate fields. The physiologist in industry is in no way isolated either within his organisation or from his academic or industrial competitor colleagues. He is encouraged to attend meetings of scientific societies at home and abroad, and to communicate the results of his efforts. He is also given the opportunity to visit academic, industrial and government research institutions in different countries.

There is no real problem with regard to the publication of biological work, but there is a tendency to defer publication until a satisfactory phase of the work has been completed rather than to appear in print with a series of progress papers. Recognition or promotion is not influenced in any way by the number of papers produced by an individual.

Although we prefer to recruit comparatively young men, about 30 years of age, it is essential that they should have had research experience. In selecting research workers we do not look for someone to solve *ad hoc* problems. We choose a man because we like his work or the type of training he has had and we expect industry to give him scope for his individual talents and that we in turn will benefit by his scientific contributions. His prospects are as good as anyone else's in reaching top management. Material advancement does not necessarily mean forsaking the laboratory for the desk. There are cases where a first-class laboratory man commands a higher salary than a member of the board. Ability to manage and lead a team of research workers is soon recognised. The rarer combination of technical ability and administrative skill in the co-ordination of the complex activities of modern industry is well rewarded by promotion and executive authority.

Enlightened industry now provides superb facilities and an attractive environment for research.

THE EDUCATIONAL BACKGROUND FOR A CAREER IN PHYSIOLOGY

By Prof. D. H. Smyth

Previous speakers have dealt with many aspects of the teaching and training of physiologists. What I should like to discuss very briefly is the problem of the type of candidate who is most likely to succeed in physiology, and the type of education which is most likely to enable him to do so. Needless to say in a question of this kind there is much room for different opinions and anything I say must be taken as my own personal point of view, rather than a universal rule for others to follow. In the first place it should be stressed that physiology is such a wide subject that it offers scope for many different faculties and skills. On the one hand there is the mathematical and physical approach, which demands considerable skill in mathematics and physical chemistry, but which has yielded a rich harvest to those capable of using these techniques. At the other extreme there are problems of animal behaviour in which an almost complete absence of knowledge of physics, chemistry and mathematics may be little handicap. Between these two extremes there is a wide range of problems demanding a great variety of skills.

In attempting to make some generalisations I should say first that physiology

might be defined as an attempt to solve biological problems by the application of physical and chemical techniques. The assumption is made that matter organised at the biological level obeys the same laws as matter comprising the non-living world, and in spite of the great complexity of living matter, there is no doubt that this approach to physiology has been an extremely fruitful one. This means that while the problems of physiology are biological, the means of solving these might be called physical; and I would like to expand a little on these two aspects of physiology, the biological and the physical.

I think that many people can be roughly divided into two groups as regards their fundamental interests; the biological and the physical. While this applies to both work and recreation, in the majority of people it is more apparent in their recreations, since few people can choose precisely the kind of work they want to do. The biological group are fishermen, gardeners, animal breeders, collectors of insects, etc. They are fascinated by the diversity of problems encountered by living things, and the ways in which these problems are solved. The physical group are those who prefer machinery and non-living things. Individuals in the physical group find their outlet in taking their car to pieces and putting it back together again, and like to spend their spare time in adjusting various pieces of electrical or mechanical equipment. They like the precision of the moving parts of the machine in contrast to the uncertainty of living things. This difference in interest may play some part, although possibly a small one, in making people take up different careers, but it is certainly likely to have some influence in contributing to success or failure in their careers. In the case of physiology I think the biological group are better equipped to see real biological problems. They have the instinct for thinking biologically and visualising the problems of living matter. In spite of their interest however they may lack the techniques for analysing the problems which interest them. This analysis very often requires the approach of the physical scientist, and the person whose school training has been entirely biological may be at a great disadvantage when it becomes necessary to penetrate more deeply into what happens in living matter.

For this reason it appears to me that training in physics and chemistry at school is an essential part of the equipment of the future physiologist. The person with good physics and chemistry on entry to the University is in a much stronger position to begin the detailed analysis of biological phenomena. Indeed the candidate who specialises in zoology and botany at school to the exclusion of the physical sciences may be in a position to get little profit from an Honours School of Physiology. Needless to say, a good deal will depend on the teachers in the schools, and I am thinking now primarily of the position where biological teaching is mainly descriptive and observation.

I should like at this point to make a criticism of an attitude which I believe exists in a number of schools. If a pupil shows proficiency in physics and chemistry he is encouraged to make a career in one of these subjects. On the other hand, if his physics and chemistry are weak, but he still wants to do something on the scientific side, he is encouraged to take up medicine or one of the biological sciences. I have seen repeated evidence for this point of view in interviewing applicants both for Medicine and for the Honours School of Physiology. I believe it is not fully appreciated in the schools what a scope is offered by a career in biology or medicine to those equipped with a good knowledge of physics and chemistry, and of the biological sciences this applies particularly to physiology. It is of course appreciated that physics and chemistry form part of the First M.B. course and that most students taking a biological subject will have done physics and chemistry at least to ordinary level, but in spite of this I believe that many schools regard biology and medicine as the proper spheres for those whose physics and chemistry are not quite up to standard. I would therefore stress very strongly the importance of

physics and chemistry as a preliminary to a course in physiology. Indeed I would say that if I had to choose between two applicants; one who had no physics and chemistry at advanced level, and one who had no botany, zoology or biology at advanced level for a place in a physiology course, I would invariably choose the one with the training in physics and chemistry.

Having stressed the importance of the physical sciences in physiology I would like to consider the biological interest. However well equipped a person is with physical and chemical knowledge, he will only become a successful physiologist if he has a driving interest in biological problems. How this interest can be aroused is not easy to say. I certainly think it can develop in those who have done no biology in schools, although I am sure it is likely that good biology teaching in school will stimulate this interest. Without this real biological interest the outstanding physiologist can never be produced. Without it there is also a very real danger of that occupational hazard of all physiologists, i.e. of becoming a purveyor of gadgets instead of solving real problems. I even think there may be a greater danger of this, the greater the skill in the physical sciences. The person interested in a biological problem may try to formulate his results as clearly as possible by expressing them in a mathematical form. Obviously the more mathematics he knows the better chance he has of doing this, but there is a danger sometimes of getting interested in mathematical juggling for its own sake, and at the end of the process the original biological problem may not be any more clearly illuminated.

In conclusion, I would stress the importance of a good background in the physical sciences. Let the schools see that some of those with an interest in biological problems are not too easily diverted from the more rigorous discipline of the physical sciences, and let them send up to the University some pupils well equipped in these respects. Having got to the University, this knowledge of the physical sciences needs to be increased and strengthened, but along with it must go a fostering of interest in biological problems. The successful physiologist will be one who sees biological problems he wants to solve, and who is prepared to equip himself with the necessary background of physics, chemistry and mathematics to achieve his aims.

DISCUSSION: PHYSIOLOGY IN AMERICA

By Prof. R. W. Gerard

The physiological *population* in the United States and Canada consists of some 2,000 core physiologists and 6,000 physiological scientists. These constitute about one-third of all biologists and a thirtieth of all scientists. Two-thirds of them are animal physiologists, the others are in the plant and bacterial areas. In 1953 there were 1,300 animal physiologists, the closest parallel to the British Physiological Society membership. Of these, over 90 per cent have the Ph.D. or the M.D. (one-third have the latter, but the fraction is declining); and about one-fourth each are concerned with neurophysiology and circulatory physiology.

The *field* of physiology lies clearly between the academic and the applied, on all criteria. Men have entered it from practically all departments and leave for some ninety outlets—about twenty in basic science, thirty in medical science and related, thirty in industry, and ten in extra-academic education.

In *occupation*, two-thirds of all physiological scientists are in academia and 55 per cent of their effort is devoted to research. Research projects cost about $14,000 per man per year and result in an average of one publication per project. Over 80 per cent of the physiologists are quite satisfied with their positions, although most would like about one-third additional income. This averaged $6,700 for all physiological scientists, $7,600 for the medical group.

Some clear *trends* are apparent, indicating the extremely rapid development of science. Whereas the U.S. labour force has doubled over thirty years, the population of scientists has quadrupled and that of physiologists has increased eightfold. Nevertheless, there has been a recent lag in the output of Ph.D.s in physiology, indeed in biology generally; and within the area of physiological science the young men are tending into the field of bacterial physiology. This decline is unfortunate, for the future need is demonstrably great. For physiological scientists as a whole, the need per year is judged at 700 and the Ph.D.s anticipated, 200. For core physiology, the need is 200 and the Ph.D.s expected, only 100.

These findings suggest the need for better *education and public relations* in this area, for the lag is certainly attributable to a poor lower education and to very inadequate public understanding. Teachers of biology have had little or no science ('The students have limited training and sometimes none in the subject they later teach; a fourth of the elementary teachers in Iowa had a bachelor's degree and fewer than half the science teachers had even a minor in their subject area.' *Mirror*, page 224); and of the teachers of physiological science, almost two-thirds have no contact whatever with professional physiology or physiologists. The public seldom hears the word 'biology', let alone 'physiology'. The mass media credit all biological advances either to the physician or the chemist, and 'physiology' if it means anything, suggests a sort of do-good physical hygiene.

It is clearly timely to have a Physiological Society session on Education and Careers in physiology; and to have it in conjunction with the British Association, concerned with bringing physiology and all science to the public. I am grateful for the opportunity to bring to your attention our findings of the similar situation in the United States; and again express the hope that some joint activities may come into being.

THE NON-CONSERVATION OF PARITY[1]

by

PROFESSOR J. C. GUNN

THE discovery that parity is not conserved in certain weak decay interactions was made early in 1957 following a brilliant suggestion, made the previous summer, by the American theoretical physicists Lee and Yang. The suggestion has since been verified by repeated experiments, both on nuclear beta-decay and on the decay of the unstable particles, artificially produced or found in the cosmic radiation. Simultaneously, the exact way in which parity is not conserved has been described by a very complete theoretical formalism. The time is thus quite a suitable one for reviewing the significance of this work, which may fairly be said to have introduced the greatest revolution in our thinking about space and time since the enunciation of the Special Theory of Relativity by Einstein in 1905.

We are going to be dealing with elementary particle interactions. Accordingly, while we have no need of any complicated picture of atomic or nuclear structure, it is useful to recall the four fundamental interactions which, so far as we know, are responsible for all physical and chemical processes. These are:

(*a*) Nuclear interactions—the strong short-range forces (range of the order of 10^{-13} cm) binding together the neutrons and protons which make up atomic nuclei. The forces are transmitted by mesons of various sorts, but most importantly by the π-meson or pion.

(*b*) Electromagnetic interactions—these are responsible for the forces between charged particles and hence control all atomic and molecular structure. In this way all the ordinary processes of chemistry, and indeed of living systems, are governed by electromagnetic interactions. The radiation of light by atoms is the fundamental electromagnetic process.

(*c*) Gravity—the force of gravity only becomes dominant when we are dealing with very large bodies. It is the force controlling the motion of bodies on the astronomical scale, but can be largely neglected in problems of elementary particles.

(*d*) Weak decay interactions—the typical process here is nuclear beta-decay by which, for example, a neutron transforms into a proton, simultaneously releasing an electron and an anti-neutrino. The process is written

$$n \rightarrow p + e^- + \bar{\nu}. \qquad (1)$$

It is, of course, arbitrary that the 'little neutral particle' emitted in the above β-decay is called an anti-neutrino. The choice is made so that, counting an anti-particle as minus one particle, the number of particles on each side of (1) coincides. For an appreciation of the fundamental interactions it is important to note that they differ greatly in their strength. Taking the strength of nuclear interactions as unity, the strengths of the others are roughly: electromagnetic 10^{-2}, gravity 10^{-39}, weak decay 10^{-14}.

We now return to the hypothesis of Lee and Yang that, while parity is accurately conserved for all the 'strong' processes of physics and chemistry, controlled by nuclear and electromagnetic interactions, it is not conserved in weak decay processes. This hypothesis is rather surprising in one sense, for it runs counter to what has been one of the most fruitful trends in theoretical physics since the enunciation by Einstein of his special relativity theory. The essence of Einstein's theory is that the laws

[1] Paper delivered to Section A (Mathematics and Physics) on August 28, 1958, at the Glasgow Meeting of the British Association. Professor Gunn is Professor of Theoretical Physics in the University of Glasgow.

of physics have the same form in all reference frames moving uniformly relative to each other. All such frames are then equivalent in their fundamental description of phenomena, and it is impossible, for example, to distinguish one of them from the others and say that it is genuinely at rest. The transformation in the description of an event in space time between two reference frames is given by the Lorentz transformation. Mathematically, then, we say that the equations of physics are invariant to Lorentz transformations.

The important idea here is that of the invariance of the laws of physics to certain transformations. In combination with quantum mechanics this idea has proved a very valuable guide in the development of the present-day description of elementary particles. Various other invariance principles have been introduced, of which in particular we mention the postulate of invariance under charge conjugation, which consists in that replacement of every particle by its corresponding anti-particle. If we imagine anti-matter constructed by replacing every particle in ordinary matter by its anti-particle, then, according to the postulate of invariance against charge conjugation, physical processes in this anti-matter obey exactly the same laws as we find to hold for our ordinary matter. Were we in touch by radio with some distant planet, we could not by exchange of observations identify whether it consisted of matter or anti-matter.

It was an obvious extension from the known invariance of the laws of physics to proper Lorentz transformations to suppose that the invariance would also hold for reflections in space and time. The parity transformation is in fact reflection at a point in space. It may be considered either as an active or as a passive transformation. In the passive transformation the object being viewed is left undisturbed but we view it from the reflected reference frame so that the point $P\,(a, b, c)$ in S becomes $(-a)$, $-b$, $-c)$ in S′. Alternatively, we can make an active transformation and reflect each point of our physical system in the origin so that the point P of the system is transferred to P' which in S has coordinates $(-a, -b, -c)$. Clearly the processes are equivalent, but it is often more vivid to think in terms of active transformations.

At this point it is probably useful to distinguish between a parity transformation, which is reflection at a point, and the more familiar reflection at a mirror, which is reflection at a plane. The distinction between these two is perplexing at least to some students of elementary physics who have been puzzled on being asked at an oral examination why, when they look at themselves in a mirror, they do not see themselves upside down, as well as with right and left hands interchanged. If the mirror is in the zx-plane say, then reflection in it means

$$(x, y, z) \rightarrow (x, -y, z). \tag{2}$$

If we now rotate through 180° about the y-axis we leave y unaltered, but change the signs of x and z. Thus a parity transformation is compounded of (a) reflection in a plane, (b) rotation through 180° about the normal to the plane. As the equations of physics are certainly invariant to rotations in ordinary space, invariance to a parity transformation is thus in fact the same thing as invariance to reflection in a mirror.

We are now in a position to say precisely what it means when we say that the laws of physics are invariant to a parity transformation. It means either that in the reflected reference frame S' the laws of physics are of the same form as in S, or, what is the same thing, that the reflection of any physical process in a mirror satisfies the same equations of motion as are satisfied by the real process in the laboratory with us. The mirrored image of any physical process is one that could take place, in accordance with natural laws, in the real world—or, putting things in graphic terms, the scientist looking at an experiment cannot tell whether he is looking at the real thing or at a looking-glass picture—we cannot discriminate whether we are looking at the real or the looking-glass world.

Now it has been abundantly demonstrated for the strong interactions of physics—

the electromagnetic interactions responsible for atomic structure and hence for all the physics and chemistry of everyday life, and the nuclear interactions responsible for nuclear structure, which now play their part in atomic bombs or nuclear power—that these strong inter-actions do conserve parity (or, if they fail to do so, it is in an undetectably small way). Thus the mirror image of any experiment in electromagnetism, or in interaction of nucleons, is a physically possible experiment. Let us take one simple but revealing example from electromagnetism—suppose we have a current running round a circular conductor in a counter-clockwise direction looked at from above, i.e. the current is forming what is called a right-handed screw with the upward vertical direction. The magnetic field at the centre of the coil is along the upward vertical, and a unit positive magnetic pole placed there will be forced upwards. Consider now the image of this experiment in a vertical mirror. The mirror imaged current is in a left-handed screw round the upward vertical so that the magnetic field is downward (clearly in the mirror image the sign of charge is not altered). However, as the mirrored magnetic pole still goes upwards, we are led to the conclusion that the mirror image of a positive pole is a negative pole. Subject to this conclusion, the mirrored experiment is a perfectly valid one and indistinguishable from a real experiment in our laboratory. Incidentally, this experiment shows why we cannot, with the help of electromagnetism, give an absolute definition of what is meant by a right-handed screw.

At this point we may note that in our local environment on Earth, it often happens that an object exists a mirror image of which either doesn't exist or is found only very rarely. We can, with Kelvin, call an object 'chiral' if it doesn't coincide with its mirror image or rather if its mirror image cannot be made to coincide with the original object by translations or rotations in space. As implied by the name, the simplest chiral object is our hand—the mirror image of a right hand is a left hand, and to make our left hand coincide with our right we should need to be able to interchange its back and front. To take a homely illustration, a pair of gloves are 'chiral', but a baby's mittens are identical with their mirror image. Consideration of this example shows what we need to have for an object to be chiral—there must be a plane in the object in which is specified a direction of rotation, and there must be a normal direction to the plane (corresponding to the back and front of a glove or a hand). Thus the fundamental chiral object is the screw which has already been considered in the electromagnetic experiment.

Now in nature we certainly find chiral objects—for example, in the inorganic world there are right- and left-handed quartzes. However, in the inorganic world it is roughly true to say that right and left-handed specimens occur with approximately equal frequencies. This is, in a sense, a coincidence—it simply means that for inorganic systems the environment is in general not strong enough to force a definite handedness (or chirality). Thus, for the inorganic world we can say that the mirror image is not only in principle possible but that it actually occurs.

In the biological world it is clearly not true that the mirror image of each system exists—men are mostly right-handed, with their hearts on the left side, and no doubt on the morphological scale there are many other phenomena of handedness with which a physicist is not familiar. Equally on the biochemical scale amino-acids, enzymes and nucleic acids like the famous D.N.A. exist only in a single left- or right-handed form. It is very interesting to speculate how these right- and left-handed properties of biological systems have arisen, and indeed how far handedness at a biochemical and a morphological level are linked, but this would take us too far from our subject. In each case, we may say that natural selection has played its part in selecting the left- or right-handed biological strain to survive, so ensuring a stable biological system. At any rate, we need have no feeling that the absence of opposite-handed biological forms is anything to do with non-conservation of parity. The opposite-handed forms can exist in principle but simply do not happen to do so.

We are now in the position of appreciating just what parity conservation means, and as physics is an art as well as a science, we might well feel that it represents a symmetry we would want nature to preserve. However, ultimately physics is an empirical science, and the facts of β-decay, which we must now discuss, are plainly that parity is not conserved in this type of interaction, just as proposed by Lee and Yang.

The first experimental demonstrations of parity non-conservation was that carried out by Wu and others on the β-decay of Co^{60}. In this decay one of the Co^{60} neutrons decays into a proton as in the fundamental process (1) and we may write

$$Co^{60} \rightarrow Ni^{60} + e^- + \bar{\nu} \text{ (spin } 5 \rightarrow 4). \qquad (3)$$

The spins here give the nuclear angular momentum in terms of the fundamental unit of angular momentum (Planck's constant divided by 2π). The Co^{60} nuclei have a magnetic moment associated with their spin, and, with the help of the techniques of low-temperature physics in a strong magnetic field, the magnetic moments can be used as a handle to orient the nuclear spins, so that they point in a given direction, say upwards. This means that the nuclear-spin angular momentum has the same sense as that of a top spinning in a right-handed screw about the upward direction. We can ignore the practical point that for a given strength of magnetic field there will only be a preferential alignment of nuclear spins along the chosen direction and can assume that the nuclei are perfectly aligned. Wu then looked for the decay electrons—they were found to be preferentially emitted in the downward direction opposite to the direction of the nuclear spin. This experiment immediately shows that parity is not conserved in β-decay. For consider its image in a vertical mirror. The mirror image of the Co^{60} nuclei spinning about the upward direction in a right-handed screw are Co^{60} nuclei spinning in a left-handed screw about this direction, or, in other words, with their spins oriented downwards. The electrons in the mirror image are still preferentially along the downward direction. Thus the mirror image shows Co^{60} nuclei emitting electrons preferentially along their spin direction, which is contrary to what has been found to be true in our real laboratory experiment. It might, of course, be objected that we are again being deluded by an environmental phenomenon. However, this would imply a complex structure for the neutron and proton quite at variance with all our ideas and other knowledge about fundamental particles, according to which the mirror image of a Co^{60} nucleus differs in no way from the real Co^{60} except in its spin orientation.

In terms of our earlier discussion, it is clear that the parity non-conservation shown in β-decay must be identified with some handedness shown by nature, and it is not unnatural to expect that the neutrino, the particle especially associated with β-decay, will prove to be the source of this handedness. A more detailed study of the β-decay problem confirms this expectation. In order that angular momentum may be conserved in the reaction (3) it is necessary that the components of angular momentum of the emitted electron and anti-neutrino should total unity along the chosen upward direction. Consider the most frequent case when the electron is emitted downwards. The electron has spin $\frac{1}{2}$, so this will contribute $\frac{1}{2}$ along the upward direction if the electron has left-handed spin along its direction of motion. This is indeed confirmed to be the case. The total correct spin will then be found, either if the $\bar{\nu}$ has left-handed spin and is emitted in the same direction as the electron, or if it has right-handed spin and is emitted in the opposite direction to the electron. In a decay of this sort (Gamow–Teller selection rules) these directions of emission of the anti-neutrino are given respectively by the two theoretically possible β-decay interactions known as T (tensor) and A (axial vector). Very beautiful and elaborate experiments have decided the issue in favour of the axial vector interaction which means that the $\bar{\nu}$ emitted in the β-decay of Co^{60} goes dominantly in a direction opposite to the electron and has right-handed spin. Handedness in all β-decay

phenomena is now ensured if we assert that the anti-neutrino always appears as a particle with right-handed spin, while the neutrino, correspondingly, has always left-handed spin.

A very elegant neutrino theory has been developed to incorporate the above properties. We shall note only a few of its consequences. In the first place, it can consistently be asserted that the neutrino always has left-handed spin about its direction of motion only if the neutrino has zero rest mass, and so, like a photon, travels always with the velocity of light. Otherwise, we could view the neutrino from its rest frame and a spin orientation would then be impossible. Again, the proposed neutrino theory violates the postulate of invariance under both the parity transformation, P, and charge conjugation C. For the parity transformation carries the left-handed neutrino over into a right-handed neutrino which does not exist, while charge conjugation carries the left-handed neutrino over into a left-handed anti-neutrino which is equally non-existent. However, though P and C are separately not conserved, their product PC may be conserved, for this converts the left-handed neutrino into the right-handed neutrino which does indeed exist. The presently favoured form of neutrino theory does in fact yield PC conservation, and this is in accord with present experimental evidence.

In the light of these discussions it is interesting to see what modification has to be made in some of our earlier statements based on parity conservation. We then asserted that, from observation of an experiment, we could not tell whether it was genuinely in the laboratory with us, or was a mirror reflection, or equally that it was impossible to convey to a distant observer what we meant by, say, a right-handed screw. Apparently we can now do both these things. If the experiment is a β-decay one, it is immediately distinguishable from its mirror image, and to define a right-handed screw we have only to ask our distant observer to look at his anti-neutrinos. However, closer thought shows that the question is linked with that raised earlier regarding our ability to distinguish between matter and anti-matter. If, as suggested above, PC is conserved, then the mirror image of an experiment with antimatter is completely indistinguishable from the experiment in our laboratory with matter. Our distant observer may be living on the planet of another star made up of anti-matter. His neutrinos will then have right-handed spin, and we shall mislead him if we tell him that neutrinos are left-handed. In other words, neutrinos only give us the power to discriminate between right- and left-handed so long as we know that we are dealing with ordinary matter. The existence of anti-matter in large amounts is a matter for astrophysical speculation. Current estimates make it appear unlikely that there are any anti-matter stars in our galaxy, so, at least in this fairly wide environment, we can probably assert that neutrinos are always left-handed.

We must finally note that the phenomenon of parity non-conservation has been confirmed to hold throughout the complete range of weak decay processes—for example, in the chain of decay of the π-meson:

$$\pi^+ \rightarrow \mu^+ + \nu \quad ; \quad \mu^+ \rightarrow e^+ + \bar{\nu} + \nu. \tag{4}$$

In the first of these decays, if we suppose the π^+-meson at rest, the μ^+ and ν go off in opposite directions, each with left-handed spin. When the μ^+ decays, the positrons are emitted dominantly backwards along the direction of motion of the μ^+ with right-handed spin. Parity non-conservation has equally been found in the decay of the new 'strange' particles found in high-energy processes, such as the hyperons. No neutrinos are involved in these latter decay processes—for example:

$$\Lambda^0 \rightarrow p + \pi^-, \tag{5}$$

so that we have the interesting phenomenon of parity non-conservation without the intervention of neutrinos. However, the time of the decay is so like that to be

expected from a coupling of β-decay type that an interaction of this general type (known as a Fermi-interaction) is believed to be responsible. It must also be noted that the question is still outstanding how far parity conservation will be confirmed to hold perfectly in all strong interactions. In particular there is still much uncertainty about the parity of the κ-mesons. However this may be resolved, it remains true that no measurable effect of parity non-conservation has yet been found in nuclear or electromagnetic interactions.

In conclusion, the discovery of parity non-conservation has focused our attention on the most interesting and extraordinary properties of the neutrino. It is like the photon in having zero rest mass and moving always with the velocity of light, discarding one of its possible polarisations. However, unlike the photon, it has an unimaginably small interaction cross section with matter (of the order of 10^{-43} cm^2), so that a neutrino, once created, is essentially never absorbed. Its lifetime before absorption in our galaxy would be of the order of 10^{26} years, and correspondingly longer in intergalactic space. Neutrinos are, of course, not rare particles. They are being abundantly created in the hydrogen-burning process in stars:

$$p + p \rightarrow d + e^+ + \nu, \tag{6}$$

as well as in radioactivity. Perhaps now that their role in individual nuclear processes is so well established, effort will be stimulated to understand better their role on the cosmological scale.

OPERATIONAL RESEARCH IN ADVERTISING[1]

by

J. MAITLAND

General

1. Advertising is rather like the *Arabian Nights*—there are a thousand and one methods which can be employed with advantage, but all of them have as their sole objective the movement of the demand curve from position D_1 to D_2.

This statement covers practically all forms of commercialised promotional activity whether it be prestige or institutional advertising, appeals for charitable

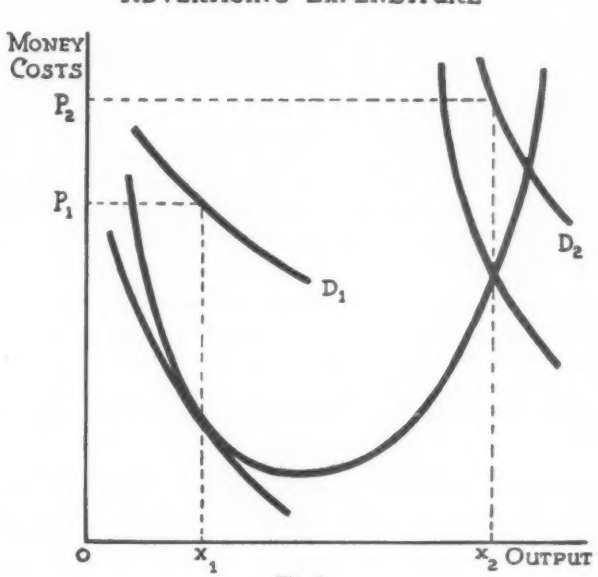

Fig. 1.

organisations, or a straightforward sales plan designed to sell goods or commodities. This change in demand reflects not only an increase in profits as a result of extra sales, it can also reflect an increase in the number of recruits for the Services, the nursing profession, or the National Coal Board; the membership of a political party, or investment at formidable odds against you in a football pool!

From this simplified statement let us go behind the scenes and examine some of the mechanisms involved in the shift of the demand curve.

2. The methods hitherto mainly used to assess the effectiveness of a campaign have been based on costing techniques, the use of break-even charts, and any

[1] Paper delivered to Section F (Economics) on Thursday, August 28, 1958, at the Glasgow Meeting of the British Association.

statistical method that looked as though it were useful. Over the course of years some useful and some downright odd results have been obtained. Readers of Mr. Darrell Huff's entertaining little book, *How to Lie with Statistics*, may remember some of the interesting correlations which have been made between the production of false teeth and the salaries of school teachers; the number of undergraduates in a university and the number of inmates in mental institutions.

For those who are interested in adding to their own collections of Curious Correlations, mention may be made of a recent statement by a correspondent to *The Lancet* which purported to show that there is a statistically significant correlation between the toxaemia of pregnancy and membership of the Baptist Church! But as we also are interested in causes and effects it is necessary to describe the basis on which some of these new techniques might be exploited.

3. Since the war scientists all over the world have been developing and applying new techniques in the design and analysis of experiments called Operational Research. There appears now to be a consensus of opinion that there are certain steps which have to be taken before a solution can be found by these methods. The *first* stage is devoted to a description of the problem in quantitative terms. The data obtained is then analysed and a model is produced which describes either mathematically or symbolically the structure of the problem—and this is the *second* stage. The *third* and critical stage is the use of the model in practice.

4. These techniques and some of their war-time applications were first given public prominence in an address to the British Association by Prof. Blackett in 1948. As these techniques have now to a large extent become dependent upon mathematical methods of solution it may be of interest to point out one of the methods employed by another branch of applied science—that of bacteriology. The original reference goes back to 1890, when a German doctor, Prof. Robert Koch, enunciated a series of conditions which must be fulfilled in order to establish a causal relationship between infection and disease. In general terms, these 'postulates' as they are called run as follows:

(*a*) The organism should be found in all cases of disease in question, and its distribution in the body should be in accordance with the lesions observed.

(*b*) The organism should be cultivated outside the body of the host, in pure culture, for several generations.

(*c*) The organism so isolated should reproduce the disease in other susceptible animals.

Even on a superficial inspection of these two methods a similarity between them can be detected. We can go one stage further and following Keynes (Treatise on Probability) we can state that there is a *positive analogy* between them.

The Analysis of a Campaign

Before we can use Operational Research in advertising we must construct a method of analysis that is applicable to the data. It is now time to recall the *Arabian Nights* and to emphasise that if there are a thousand and one ways of advertising there could be a thousand and one ways of analysing the results.

The analytical method which we shall use, has its origins in the past, even before the days of Aristotle; its contemporary application is in high-speed computing machines. The method is that of analogy and we may define it as a form of inference in which it is reasoned that if two (or more) things agree with one another in one or more respects they will (probably) agree in yet other respects. It is now common in attempting to construct a theory to explain observed phenomena, especially where chains of causation have to be investigated, to make use of analogies or models. The

possible inter-relations are more easily thought of in terms of pictures—in this case they are called iconic or pictorial models. The analogue model which we shall use, employs one set of properties to represent another set of properties in the structure being explored. There are dangers in the unselective use of models as Braithwaite (1953) has pointed out, in that, for example, attributes peculiar to the model may be erroneously imported into the deductive system—but if these dangers can be avoided the method is productive and powerful.

It has been stated (Keynes) that almost all empirical science rests on the methods of Analogy and Induction. In this paper we shall try to demonstrate how the resemblance between phenomena observed in various branches of science can be used to analyse the results of advertising campaigns.

In attempting to use the method of analogy it is necessary to stress that *modern* probability theory discriminates between:

(1) positive and negative analogy;
(2) total positive and known positive analogy;
(3) total negative and known negative analogy;
(4) sub-analogies, which are positive for a subordinate class, but negative for the class as a whole.

The aim of the usual argument from analogy is to generalise from one part to another of the *total* analogy which observation or experiment has shown to occur between certain selected instances.

The present study concerns the relation between the amount of advertising and the response to it, and in explanation of this possible cause and effect a number of different analogies have been developed. It must, however, be emphasised that the analogies are employed only as sources of ideas as to the kind of mathematical curve which might be used to express the relation between the amount of advertising and the response to these amounts. What we are trying to do, is to fit a smooth curve to some crude observations; and in such an operation it is legitimate to anticipate the possible shape of these curves.

The Stimulus and its Measurement

It has been asserted for some time, in the vernacular, that 'what goes in must come out'. For the economist the concept can be reduced to two terms 'input' and 'output'. But there are many other branches of experimental science which also use the concept of a stimulus and the measurement of its response.

The various branches of science upon which we shall draw for instances of analogy are:

(A) *Physiology*—with particular reference to the electrical stimulation of nerve tissue.
(B) *Pharmacology*—with particular reference to the role of biological assay.
(C) *Physics*—with particular reference to the charging of an electrical condenser.

(A) In *physiology* the study of the electrical properties of nerve fibres constitutes one of the fundamental forms of investigation. It is fairly safe to say that there are few forms of animal or insect life whose nerve properties have not been investigated. Hill, in Great Britain, for instance has been able to show an interesting *linear* relationship between the 'input' or stimulus current and the response of nerve tissue. Solandt, in Canada, has confirmed this relationship in his work on a number of nerve preparations from lower vertebrates.

In simple terms this means that, for any number of stimuli, there are certain response patterns which are common to each group of animal or insect. There is,

however, one response pattern which is so important that we must enlarge upon its behaviour. If two electrodes are connected to a nerve and to a galvanometer as shown in Fig. 2 (*a*) and if one short stimulus is given to the nerve, the galvanometer will show that electrode *B*, the one nearer to the point of excitation becomes negative for a very short time. For a frog, the numerical factor for the sciatic nerve is of the order of 0·00217 sec. Very shortly afterwards the more distant electrode *A*

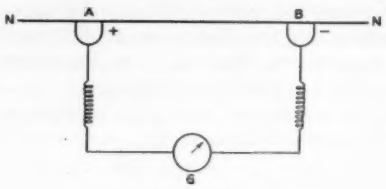

Fig. 2 (*a*).

becomes negative to *B*. This first and major deviation of the flow of the current is called the *action potential.*

Monnier and Coppée in France by careful experiments, also on the sciatic nerve of the frog, stimulated the largest *alpha* fibres by brief shocks and obtained the following type of picture, Fig. 2 (*b*).

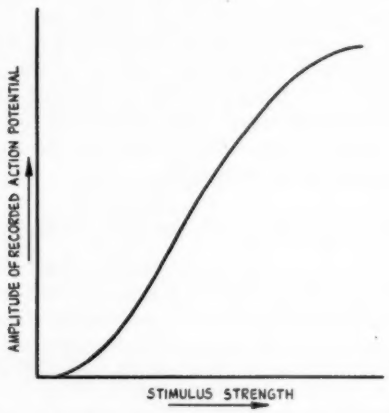

Fig. 2 (*b*).—Diagram to show the type of electrical response obtained by stimulating nerve fibre.

We shall meet this type of curve again. The points which I would like to stress are:

(1) the concept of stimulus and response, and
(2) this particular type of curve which is obtained as a result of graded or increasing stimuli.

We will now try and apply it to advertising. If the analogy of the physiological response to stimulae were to hold good we should expect a linear relationship between the response (*R*) and the natural logarithm of the advertising input (*A*).

In the following table column 3 shows the ratio of *R* to $\log_e A$; and the gradually rising value of this ratio to the increased stimulus suggests that such a relationship may exist.

TABLE I

Input units (A)	Response units (R)	$\frac{2}{1}\log_e R/\log_e A$
(1)	(2)	(3)
220	51	9
477	72	12
1,400	169	23
2,000	140	18
3,700	315	38

It should be noted that for reasons of industrial security actual units have been concealed by multiplication by constants to prevent identification of amounts. Another campaign yielded the following results:

TABLE II

Input (A)	Response (R)	$\frac{2}{1}\log_e R/\log_e A$
(1)	(2)	(3)
240	30	5
330	75	13
540	103	16
1,250	162	23
3,000	267	33

In Figs. 3 and 4 the response values for Tables I and II have been plotted against the input on a logarithmic scale and linear functions (shown as broken lines) have been fitted by least squares to these points.

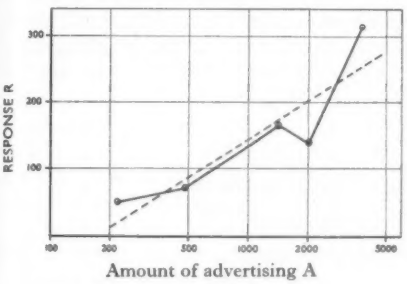

Fig. 3.—Fitted curve is
$R = 84{\cdot}06 \log_e A - 432{\cdot}6$.

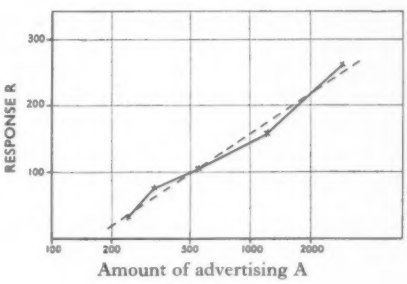

Fig. 4.—Fitted curve is
$R = 87{\cdot}88 \log_e A - 447{\cdot}5$.

These response values are encouraging, but it would be necessary to accumulate considerably more data than we have been able to collect in order to establish stable response values.

(B) In *pharmacology* the interpretation of the quantitative response to a series of graded stimuli is one of the basic methods of assessing the purity and strength of a wide variety of therapeutic and toxic substances.

Some idea of the importance of this technique, which is variously called bioassay, probit analysis or the probit transformation, may be gained by knowing that during 1957 some three million animal experiments were performed and of this total some 37 per cent were concerned with experiments using bioassay techniques. This is a substantial number by any standard.

We are, however, immediately concerned with one aspect of these experiments.

What is the graphical form of the response obtained? When the percentage mortality is plotted against the dose of the drug an approximately S-shaped curve is obtained. The results of an experiment used in the standardisation of a cardiac stimulant called Strophanthus are shown in Fig. 5.

This S-shaped curve is called by statisticians the sigmoid curve and its importance for students of advertising cannot be overestimated. Wherever you find a sigmoid growth curve, there also do you find stability, equilibrium and a *normal rate of growth.*

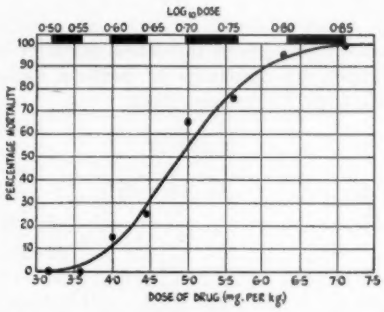

Fig. 5.—Probit transform showing the mortality curve obtained after the injection of graded doses of Strophanthus to a group of rats.

(C) In *physics*—the charging of a condenser is usually considered by schoolboys to be a fairly simple operation—it is in fact child's play. By contrast the analysis of the behaviour of the current is anything but simple—and depends upon consideration of such factors as, the strength of the current, the potential difference, and the charge on the plates of the condenser.

We can begin our analysis at a point somewhere between these two extremes. We will, in fact, begin by asking what kind of response do we get by charging a condenser. When we plot the strength of the current against time we obtain the following type of response. Fig. 6.

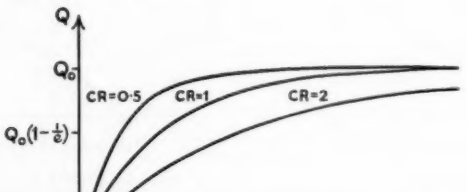

Fig. 6.

You will notice at once that we have seen this type of curve before, and we shall therefore do a little further exploring. We can now link up some of our previous inquiries with some recent work in America by Vidale and Wolfe (1957).

Perhaps this is the right time to stress that because the science of physics is an exact science you may be expecting us to handle exact units. Those of you who have attempted any research in advertising will have been bedevilled by the search for stable, measurable units. May I set your minds at rest—none have yet been found. In an idealised, controlled experiment, the goods advertised would be labelled in the same way that radio-active isotopes can be traced through their life-cycles.

Experiments can of course be made to conform to the rigid limits of control which physics demands. But their very rigidity would be their undoing, for conditions as rigid as those do not exist in economics. The nearest approach I can offer to real life conditions were the activities of certain gentlemen who recently decided to raid a bank and found to their disgust that they had not stolen notes bearing the imprint of the Bank of England. The notes belonged to one of the East African federations and were only legal tender in those countries. In due course the notes were found, and so were the enterprising 'entrepreneurs'. The lesson that they had failed to learn was that all notes are serially numbered and that a little extra care in counting would soon uncover the identity of the person paying them in. If they had in fact controlled the release of the notes they might have remained undetected.

It is with the idea of 'controlled release' that we can consider one use of physics in advertising. We have just noted that in charging a condenser the voltage *increased* in a certain regular pattern. After a certain time, i.e. at the top of the sigmoid curve, the curve tends to flatten out—this position we will call the *saturation level*; when the current is switched off and the condenser is allowed to discharge, the voltage drops in a defined pattern which is known as an *exponential decay*—this phenomenon we will call the *sales* decay.

Vidale and Wolfe who have used this analogy in their study of commercial operations in America write, 'We have found that test promotions, when carefully designed with experimental controls, and on sufficiently large scale, give results that are both significant and reproducible, though the degree of accuracy attainable is smaller than ordinarily considered acceptable in many other fields of research'.

The point which we would like to emphasise is, that despite the crudity of the data obtainable it is possible by means of analogues—in this case the critical point is the determination of the saturation level to discover which are the most effective forms of advertising, for how long should the campaign last and how should the campaigns be adapted to achieve the maximum results as compared with the pattern of the analogue?

Science in Advertising—The Future

Although there has been very little published on the use of Operational Research techniques in advertising to justify any forecasting of future developments, their success in other branches of industrial and military research will, we feel, pervade the promotional field before long.

Before we can state what form or constitution an Operational Research team should take, we must briefly consider what industry needs from its sales, market research and advertising departments.

Industry needs:

(1) An indication of the future trend, either short- or long-term, of the general picture of commercial activity, with special reference to its own product. For this an *economist* is needed.
(2) An indication of the reception by the market to new products, or alternatively, what changes in public fashion need to be incorporated in new products or new designs. For this a *psychologist* is needed.
(3) An indication of the number of units which any one factory could profitably manufacture for, say, the next 12 months. To provide this indicator a *production engineer* should use the data provided by indicator (1), the economist.

Each member of the team must formulate his statements by working through stages (i) and (ii) which are: (i) the description of the problem in quantitative terms, and (ii) the production of a symbolic or mathematical model.

The advertising director now appears on the scene. It is his responsibility to use the data provided by his colleagues. By now he will have available the results of inquiries into the price of his product and its market reception. He will have:

(1) To devise a successful campaign to launch the product.
(2) To forecast the probable response to his campaign in terms of sales. For upon the success of his forecast will depend the fortunes of his factory or business.

The attainment of accuracy in forecasting varies according to the data available and to the amount of research which has been done on the problem. Thus forecasting in economics appears to be on a par with roulette and vingt-et-un; whereas forecasting in ballistics is both highly developed and accurate.

The forecasting of the weather, for instance, is being much improved by the use of numerical processes.

But modern techniques in forecasting have one common factor; scientists are constantly trying to reduce their data to a form in which they can be processed by machines. We can therefore reasonably call our new development—*the engineering of consent.*

Use of Simulators in Advertising

In trying to elucidate the complex relation between advertising effort and its effects we have found it valuable to consider analogies with other systems. These analogies have led to the formulation of expressions relating advertising stimulus and response, which show encouraging agreement with observed results.

It is natural then to use an analogue computer to obtain particular numerical solutions of the expressions for specified values of stimulus, and to hope that an analogue simulator would lead to improvements in the original expressions and, ultimately, to the ability to predict the form of stimulus needed to produce a given response.

Suppose that it is decided that the expression relating response R, stimulus S, and time t, is of the form:

$$\frac{dR}{dt} = f_1(S, t, A, B) + f_2(R, t, C, D) + f_3(X, Y)$$

where f_1, f_2, f_3 are arbitrary functions, A, B, C, D constants, and X and Y disturbing parameters taking into account external influences on the responses unconnected with the stimulus.

Then, in general, an electronic circuit can be designed so that two voltages v_1 and v_2 in that circuit bear exactly the same relation to each other, and to time, that R and S have in the given expression.

A voltage v_1 of the same form as the chosen stimulus (e.g. steady intermittent, or step-function) is applied to the circuit and the voltage v_2 at some other point in the circuit is measured with a pen-recorder voltmeter. The recorder trace will then represent the way in which the response to the applied stimulus will vary with time. In practice, the time scale of the simulator would be speeded up to, say, one minute equivalent to one month.

The constants, A, B, C, D, would be represented in the circuit by resistances and condensers of fixed value, and X and Y would be voltages of chosen form, also fed into the circuit. The effect of changes in these values on the form of response curve would be found very quickly by changing the size of the appropriate components with switches.

More interesting is the fact that the form of the stimulus can be tailored to give a response curve of a particular shape on the simulator.The electrical values can

then be converted quickly into the units of the original expression with the assurance that the real response will now have the desired form.

It should be particularly noted that there is no suggestion that the simulator replaces original thought. Its results will be precisely as accurate as the expression chosen for simulation. But because the results of changing that expression slightly are so quickly and understandably displayed the probability of improving it is great.

In the solution of this equation every member of the team has a contribution to make. So far we have included an economist, a psychologist, and a production engineer. For the control of the electronic machines we must now include an electrical engineer, and for the determination of the relevant equations and their use we need a mathematical statistician.

In practice, unless the campaign is being undertaken on a major scale, a team of two scientists and a technician might well suffice.

The picture which we have been unfolding has depended for its explanation upon the use of analogy. We are now left with a final picture of a team drawn from the disciplines of pure, applied and social science. This team is engaged upon an unceasing struggle with a mass of data undigested and unprocessed, and it is their task to unravel the laws which *appear* to link societies with each other, and to govern the inter-actions of human groups.

For many years the words 'advertising' and 'campaign' have been accepted without question. Why a campaign?—and against whom is the war being waged? We may perhaps read a more significant meaning into the phrase if we now accept the possibility of a team of scientific advisers advising their commander-in-chief, or managing director, of the vital data in the struggle for industrial survival. The picture of an advertiser of the future fighting the good fight according to the doctrines of Von Clausewitz is, perhaps, not too remote from reality.

Acknowledgements

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DARWINISM TO-DAY[1]

by

C. J. PERRIN

'SIT down before fact as a little child, be prepared to give up every preconceived notion, follow humbly wherever and to whatever abysses nature leads, or you shall learn nothing.' T. H. Huxley, the greatest disciple of Darwinism and the man who supported it most vigorously in essays and in debate, with all the fervour of conviction, wrote this admonition that still provides such a fitting introduction to an essay on Darwinism. By the Victorian-coined term 'Darwinism' we mean the evolution of organisms from pre-existing forms by the natural selection of hereditary variations, during great periods of time. How true were these words of Huxley. Previously, ignoring the facts, tied by religious conviction to the Genesis story of divine Creation, men had not been prepared to be led to the abyss by evidence. Yet evolution is a scientific theory, formulated to account for the diversity of animal and plant life on the earth. But, particularly in view of the fossil record, evolution may also be termed a fact, although perhaps more in the historical than the scientific sense. What then can we say today about the theory of Evolution which Darwin propounded just a century ago?

The concept of evolution derives from the truth that the whole world of nature is not in a state of equilibrium, but is constantly changing. This truth was very early recognised by the Greek philosophers—Heraclitus, for instance, held that all nature was in a state of flux. Many features in the living world suggest the possibility of relationship between organisms both present and past. Aristotle, by far the greatest biologist of classical times, although he arranged the organisms he knew into a small number of large phyla, did not discuss their evolution. During the Renaissance there was little progress in evolutionary thought for the climate of opinion was unfavourable. It was only at the end of the seventeenth century that ideas assumed a recognisable form. In his *Histoire Naturelle*, Buffon suggested environmental action as a mechanism of evolutionary change, although he was later forced to recant the possibility of the derivation of species—first defined by Ray in the seventeenth century—one from another. But Linnaeus—the other great biologist of the next century—though he later compromised by allowing that species could have descended with modification from genera, was adamant that genera were immutable. Not until 1816 was the first full and coherent theory of evolution suggested by J. B. Lamarck in his *Philosophie Zoologique*. Lamarck reconceived the classical, static 'Scala Naturae' but modified it as a dynamic process, claiming that '"transformism", not immutability of species, was the basis of life'; and that use and disuse were the principal controlling forces in evolution. Although his theory is today viewed in a rather different light, for long it was accepted in its original sense. Lamarck has appreciated the implication of fossils and he had gauged the probable duration of time: it is significant that Darwin was himself a Lamarckist.

This then was the peculiar legacy of scientific thought which Darwin inherited when he started his voyage as naturalist aboard the *Beagle* in 1831. Only latterly had the firm and widespread belief in Spontaneous Creation and the fixity of species been questioned. It is true that in *Principles of Geology*, 1830–33, Sir Charles Lyell established the geological succession of stratified sedimentary rocks and fossils,

[1] Essay awarded first prize in the competition sponsored by *Endeavour* and the British Association. Mr. Perrin is 18 years of age and is at Winchester College.

thus advocating an influential Uniformitarian theory of the history of the world, in place of the catastrophism of Cuvier. But at the time Lyell, not himself believing in evolution, formally rejected it and therefore, although he paved the way for Darwin, he made no advance of his own.

Darwin's voyage firmly convinced him that evolution had occurred. Three sets of observations were responsible for his ensuing revolt against the immutability of species. The first was the succession of fossils to be found in the sedimentary rocks of South America. Secondly, there was the remarkable way in which living forms of closely related species replaced one another across the same continent as the nature of the habitats changed. And thirdly, Darwin noticed that although the flora and fauna of the Galapagos islands exhibited a fundamental similarity throughout the group and with those on the mainland, yet each island possessed its own characteristic and varying inhabitants. If species were immutable, and had not changed since each had been spontaneously created, there could be no rational solution to any of these mysteries.

Soon after his return, Darwin read Malthus' *Essay on the Principles of Population*, in which he repeated Condorcet's argument that some brake must be applied if man was not to breed himself to starvation. Natural Selection immediately occurred to Darwin, who extended this idea of a struggle for survival to all living organisms under wild conditions. At last he had an explanation of adaptation. Realising that no general principle that explained the evolution of animals was acceptable unless it also applied to plants, but now convinced of the truth and practicability of evolution, the result was 'one of the most remarkable attacks ever made by the inductive method of searching for facts, whatever their import'.

An outstanding naturalist with acute powers of observation and deduction, Darwin—the Uniformitarian of Biology—systematically collected facts in support of his hypothesis for twenty years. When therefore he eventually published *The Origin of Species* a knowledge of natural history which was common to the educated Victorian enabled each reader to judge the validity of the arguments for himself. Philosophy and Christianity—especially the argument from design which was widely read in Paley's *Evidence of Christianity*—both were influential against the acceptance of Darwinism. Yet science, both its method and its philosophy, was becoming fashionable. When Darwin first published his theory, a receptive and intelligent audience appreciated something which was not in fact a bolt from the blue. We have seen how scientific opinion developed until after Lamarck there were many biologists who recognised the possibility of evolution, and several who even stated their acceptance of its truth to a certain extent, although no explanatory theory was generally accepted. The majestic martialling and logic, the humility, which we find in *The Origin of Species* leaves no doubt of Darwin's originality and greatness. A mark of his genius is that his irrefutable arguments were based on so little evidence for the fact of evolution, but all are still valid and have been confirmed by every branch of modern science. Of the more striking illustrations so well known today Darwin knew nothing. It was of immense importance, however, that the climate of opinion was favourable for his theory.

'So much of the credit for the establishment of the fact of evolution has, rightly, been accorded to Darwin that it is only just that Wallace's contribution should be recognised and honoured.' For at the same time this other great naturalist was exploring similar lines of research in the Malay Archipelago. Independently and in complete ignorance of Darwin's work, Alfred Wallace had hit upon the same solution of the problem of the mutability of species. Also having read Malthus, his ideas were very like, but not idential with, Darwin's own. Hearing of this, Darwin wanted to publish Wallace's essay written in 1855, and to give up his own work but he was persuaded by Lyell and Hooker to allow his own views to be read with Wallace's essay at a meeting of the Linnean Society. At last a complete theory was

formulated that provided a rational explanation of the causes as well as the fact of evolution in plants and animals. It was based on four propositions already known to be true, and three deductions now also known to be true. They may be simply enumerated:

(1) Many more reproductive cells than ever give rise to mature individuals are produced by every organism.
(2) The number of adult individuals of any particular species remains relatively constant.
(3) There must therefore be a high mortality.
(4) The individuals of a species are not all identical but differ in small features.
(5) Some variants will therefore succeed better than others in the struggle for survival, and the parents of the next generation will be naturally selected as the most effectively adapted to the environment.
(6) There is hereditary resemblance between parent and offspring.
(7) Subsequent generations will therefore by gradual change maintain and improve on the degree of adaptation achieved by their parents.

The next year Darwin published *The Origin of Species* with a supporting wealth of detailed evidence. But on July 1, 1858, Charles Darwin and Alfred Wallace first read to the Linnean Society this formal theory of evolution by natural selection, in which, modified though it has been, biologists still believe.

Darwin had solved a greater problem than that of discontinuity—the origin of divergence. Yet, although Darwinism is still accepted, it was then founded on a tenuous basis of which the last pages of 'The Origin' show us that Darwin was himself aware. Prejudice, even among biologists, fought to stave off the inevitable, and acrimonious debate raged after the celebrated Oxford meeting of the British Association. The first phase of the attack on Darwinism concerned evolution as a fact, and quickly resulted in a victory for the Darwinists. The second phase, a more serious attack, was concerned with the means of evolution. Within two years Herschel wrote that he could not 'accept the principle of arbitrary and casual variation and natural seclection as a sufficient account, *per se*, of the past and present organic world.' Darwin anticipated many of the biological objections that were to be raised, admitting the main gaps in his evidence, yet they were gaping and posed serious difficulties. He could see that variation was the raw material on which natural selection must act to bring about progressive adaptations, but how could he explain the source of this variation? When Darwin wrote, little was known of the laws of heredity for although Gregor Mendel was to publish his genetical studies only six years later at Brno in Moravia, thirty-five years were to elapse before their significance was appreciated by Correns, De Vries and Tschermak. Darwin had only the vague notion that offspring struck an average between the characteristics of their parents. His supposition of 'Blending Inheritance' was therefore to cause him his greatest difficulty, for, as Fisher has pointed out, variation, even if inherited, would be obliterated within ten generations by 'swamping'. Following from this, another enigma for Darwin was the great variability of animals that he had observed. This he was forced to explain by a reliance on the effects of use and disuse—a truly Lamarckian approach. That he was anxious to find another explanation is certain; this is the only part of Darwin's explanation that has had to be abandoned.

The advent of Mendelism was long delayed, for recent investigations have shown conclusively that a copy of Mendel's paper was sent to all leading museums in Europe, but at that time the climate of opinion was unfavourable for the acceptance of a mechanism which allowed only gradual evolutionary changes. It provided however, just the impetus necessary to establish Darwin's theory and to gain its general acceptance. 'Blending Inheritance' proved a stumbling block, but Particulate

Inheritance, on the other hand, provided an ideal mechanism for perpetuating variance. Hereditary resemblances, it has been established, are determined by discrete particles, the genes, situated in the chromosomes of the cells; these are then transmitted to offspring in accordance with the mechanism of germ cell formation and fertilisation. The genes preserve their separate identity, remaining constant for long periods without contaminating each other. Yet from time to time they undergo mutation. This chemical change in the gene molecules alters the characters which they control. These, however, again remain constant until another mutation occurs. Furthermore, as a result of the random assortment and recombination of genes, subject to the limitations imposed by linkage, crossing-over and affinity, an almost unlimited fund of variation has now been shown to be available for all organisms reproducing sexually. Mendel carried out many of his famous experiments with plants yet they can be repeated on animals with equal success. That the theoretical genetic ratios are very rarely found in nature, however, is due to the influence of selection. Those genes which are best adapted stand a better chance of survival and reproduction than the rest.

This concept leads to another discovery of Mendel's, the fact of dominance. Fisher has shown conclusively that an inevitable outcome of this selective process is that those genes which are beneficial will tend to become dominant to their less advantageous allelomorphs. Moreover, as we shall see, the speed will depend on the magnitude of the selective advantages involved; hence it plays an important part in influencing the results of natural selection.

Mendelian genetics were initially rejected by the selectionists owing to the discontinuous and deleterious effects of the first observed 'sports' or mutations, but with the progress of knowledge it has become clear that mutation and recombination of genes provides the supply of variation on which selection acts to cause evolution in just the way that Darwin's theory requires. The requirements are exacting for, as T. H. Huxley showed, some organisms have evolved slowly, others fast; he saw that natural selection was the only mechanism that could satisfy both these demands. It is able to do so because Mendelian inheritance provides both diversity and stability.

Particulate inheritance, thus established, involves certain consequences of fundamental importance for the evolutionary problem. Firstly, the substitution of this quantitative and deterministic science for Darwin's groping notion of 'Blending Inheritance' completely rids us of his difficulties. And another advantage of Particulate Inheritance is that it removes the necessity for postulating a high mutation rate, forced upon Darwin to counter the effects of blending genes. We now know that the incidence of gene mutation is 10,000 times lower than had previously been supposed—Ford estimates the 'average mutation rate' in living organisms to be one in a million individuals, while Haldane has calculated that the rate required to balance the natural elimination of the haemophilia gene in man is 1 in 50,000. And it is desirable for evolutionary progress that this should be so. It is now realised that the wide and discontinuous mutations first observed were the more easily detected extremes of a range in which the majority exert only slight effects. They were therefore deleterious because organisms are so delicately adjusted that there can only be small and gradual changes if a harmful upset of the gene-complex is to be avoided.

Surprisingly, since chemical stability is not absolute, the puzzle about mutations is not that they occur but that they occur so infrequently; this, however, proves no handicap to our understanding of the mechanism of evolution. As Muller has expressed it, mutations occur with 'blindness and molar indeterminacy'. It is natural selection, therefore, not mutation that has governed both the direction and the extent of evolution; it has even been estimated that if mutation were to stop now, there is already sufficient variation for evolution to continue for as long

in the future as it has hitherto in the past. 'From the evidence provided by genetics, natural selection is therefore the only mechanism capable of explaining evolution.'

The concept of natural selection was the most original part of Darwin's great book, yet forty years ago there was a strong reaction against it. Criticisms were due to a number of causes, not least of which was an over-enthusiastic advocacy of it by the previous generation of naturalists, which had led to such absurd instances of alleged adaptation as flamingoes being pink so as to be concealed against the sunset. The other criticisms may be summarised as saying first that adaptations are too complex and interlocking to have been brought about by so random an agent as natural selection, and secondly that the necessary intermediate effects in the gradual evolution of such adaptations could not be advantageous.

To the first criticism, however, that the separate yet harmonious evolution of, say, the reproductive organs of both sexes of a species argued too high a degree of mathematical improbability, there is a mathematical answer. That is why natural selection has been paradoxically defined as 'a mechanism for generating an exceedingly high degree of improbability'. The second criticism, which Darwin himself anticipated, has, like every other 'non possumus' argument melted before the progress of knowledge. The electric organs of a fish, for example, have been shown to be able to locate nearby objects while still too weak to kill prey or deter predators, and were therefore adapted to be functional at every stage in their development.

One of the great merits of Darwinism is that it accounts for adaptation purely in terms of survival value. Furthermore, its fundamental requirements—variation and natural selection—can be established in the wild. 'Industrial melanism' first demonstrated by Kettlewell in the moth *Biston betularia* is by now too well known to need description, but it does emphasise the magnitude of selective advantages. As Fisher has shown, a gene conferring only a 1 per cent advantage on its possessor might be expected to increase the frequency of its appearance to 5 per cent of a population in the course of 160 generations.

In such instances as melanism, however, where powerful agents are operating we have recently learnt that high selective advantages up to 30 per cent are found in nature. Apart from polymorphism such a response is achieved in respect of multifactorial conditions and those controlled by polygenes. A good illustration of the extremely rapid propagation of an allopolyploid plant is that of the rice-grass, *Spartina townsendii*, along the south coast of England.

The selection of a particular characteristic must often involve a balance of advantage against disadvantage. A recent study by Alison has therefore been of extreme interest for he has shown that among African natives 'sickle-cell anaemia' is controlled by a Mendelian gene which, when homozygous, frequently kills the subject by thrombosis. When only heterozygous, however, it becomes positively advantageous since sickle-cells make attack by the malaria parasite impossible. A highly deleterious condition is therefore maintained at the level of 40 per cent in some tribes for there is a significant association between the incidence of the sickle-celled condition and the lack of malaria parasites in the blood, showing how 'selection is able to make the best even of a bad gene-complex'.

Despite the incompleteness of the fossil record and the circumstantial nature of the evidence provided by other branches of biology, our knowledge of the broad outlines of animal and plant evolution is now reasonably complete. During the century that has elapsed since Darwin propounded his theory, study of the mechanism of evolution has followed three main courses. Firstly, the reappraisal of Lamarck's theory. Secondly, the determination of the true sources of variation and the means by which both advantageous and disadvantageous characteristics can be transmitted uncontaminated from one generation to the next. And thirdly, but of no less importance, has been the more precise and quantitative approach to the

study of natural selection originated by Fisher—a brilliant modern mathematical exponent of Darwinism. Now, although there is still much to learn about selective agents, research on such problems as industrial melanism in the peppered moth, banding and colour of snail shells, mimicry in butterflies and sickle-cell in man has developed new techniques for the experimental study of evolution in the field. Now we can explain anatomical similarities and the evidence of fossils; the isolation of species that Darwin noticed on the Galapagos and which is so well demonstrated by the wren, *Troglodytes*, in Britain. We know how isolation can be due to geographical, ecological, seasonal or sexual causes. We can appreciate the physiological significance of the human blood groups. And we can explain adaptive radiation of, say, the whales while refuting the criticism of orthogenesis—so often brought forward as evidence against Darwinism. While there are various types of evolutionary changes which still remain unexplained, therefore, many forms of adaptation can be accounted for purely in terms of survival value—the essence of the modern theory of evolution. Perhaps of greatest significance is the improvement in experimental techniques, and a more precise mathematical treatment. The greatest merit of the present 'neo-Darwinian' theory is that, for the first time, it gives us a workable explanation of evolution susceptible of scientific verification.

One problem still remains, however, both for discussion and indeed for resolution; the conflict with Christianity. The discordant issues of Christianity with the evolutionary theory were not raised by Darwin—the practical biologist—in *The Origin of Species*; nor indeed need they have ever been debated so heatedly and irrationally. Yet many theologians feared that Christian belief would be undermined. Darwinism conflicted, or was thought to conflict, with Christianity in many ways. By contradicting the Genesis account of Spontaneous Creation it challenged the truth of the Bible. It undermined the most popular rational argument from design for the existence of God. Above all, Darwinism was believed to challenge the fundamental view that man was created by God in His own image. If he has evolved from the beasts by natural selection how did man's special attributes arise; his self-awareness, his apparent free-will, his sense of morals and of the holy, and his idea of God?

We can see therefore that of the causes of conflict between Darwinism and Christianity many have been due to unscientific or unwarranted claims. Determinism, for example, need never have been made an important issue. Yet the most serious difficulty—the fossils that show man's evolution from apes—remains an unsolved philosophical, though not a scientific problem. How does man possess a soul? It is extremely hard to maintain that moral and other values are, or could be, a product of biological evolution, or alternatively, that because they are inexplicable in terms of science, their value is only illusory. As Knox pointed out in *Absolute and Abitofhell* the Victorian 'I believe' was later corrected to 'one does feel'; and now it is 'couldn't care less'. Yet the problems for Christians and biologists alike are still as fundamental as before in this, the 'Unresolved Conflict'.

To biology, however, the concept of evolution is fundamental. Only one hundred years ago there was a naive belief in Spontaneous Creation and the fixity of species. Yet so soundly was the theory of evolution by natural selection grounded, that recent research serves only to confirm the links in its chain of evidence and the inferences to be deduced. The pearl of Darwinism was not cultured from a single nucleus. To the early pioneers, a great debt is owed. With Darwin we accept the fact that the variation of living organisms, and natural selection acting upon them together constitute the basis of evolution. But with the appreciation of Mendel's work and subsequent scientific research into the nature of particulate inheritance our views on the origin and maintenance of variation have been greatly changed. Evolution today is as much a dynamic force as it has ever been, and with the universal acceptance of the fact of evolution, focus has switched from a concentration

on the past to a consideration of the present and future. But it is only by such a survey as this that we can appreciate just what Darwin did when he first propounded his reflections on evolution one hundred years ago, for our present theory is a synthesis, derived from thesis and antithesis of the past. The significance of Darwinism to biology is equal to that in modern Physics of the formulation of the Laws of Motion by Newton, whose importance has not been lessened by relativity and the quantum theory. Its field has been extended from the explanation of the evolution of plants and animals to the study of man and his cultures. It has exerted a profound influence on his philosophies. Yet this first general principle ever to be discovered which is applicable to the entire realm of life is derived from rigorous deductions based on simple but significant observations

ECONOMICS IN BUSINESS[1]

by

RONALD BRECH

Prologue

In this paper I shall discuss the ways in which economics can make a substantial contribution to the efficiency of business operations. I shall approach the subject from the standpoint of the ideal rather than from that of practice—of what can be done instead of what is being done. I shall ignore the question of how the economist or the economics department shall fit into a business organisation. Here size of operations and types of operations play an important role. But as fascinating as these questions of organisation are, the more urgent task is to describe the ways in which economics can help business and to ascertain the limits of that help. If economics is to assist in business management, then the economist must understand and have sympathy with the business problem; ideally he should be a manager in his own right and have an instinct for business. After all, economics can be of value to business only if it contributes towards the increase in efficiency in the long run. By its very nature the contribution of economics to the minimising of cost (or the maximising of profits) is likely to be indirect—but it must be effective for all that.

Ends and Means

There is an obvious and close link between economics and business. The businessman is an operational economist even though he may not have read a single book on economics or attended a single lecture. Every day of his life he is taking decisions that involve some form of economic judgment. Indeed, economics itself started as a study of the operations in the market place by market operators of one kind or another, who tried to explain in terms of formal principles the mechanics of market transactions. Unfortunately since then a barrier appears to have been created between economics and business—a barrier in which both language and thought have played an important part. The businessman turned more towards accounting, while the economist sought refuge in the higher realms of political economy.

This artificial separation of twin interests has hurt both the business community and the economic faculty. The businessman has much to gain from subjecting his hunches and decisions to the rigours of scientific analysis. In the same way the economist has much to gain from being able to test his theoretical models in practical affairs.

Business is concerned with combining human and material resources to satisfy pecuniary wants—the demand for goods and services expressed in terms of money. In this it has to deal with both ends and means in themselves as well as with human and institutional behaviour as a relationship between means and ends. Indeed the theory of choice, based on the balancing of margins, is as important to the businessman as it is to the economist. What people choose, how people choose, why people choose, are all vital questions in business. The final arbiter for the businessman—as for the economist—is the consumer. Both must know and understand how the consumer will react to different phenomena and to changes in phenomena. It is true that in the past the economist has been more concerned with studying the reaction of a single consumer and even an idealised one at that. The businessman,

[1] Paper delivered to Section F (Economics) on September 1, 1958, at the Glasgow Meeting of the British Association.

on the other hand, must inevitably be interested in the reactions of a group, and often the largest group. And the businessman has to know not only what consumers want now, but also what they will want later on; in other words, he must have a shrewd idea of how consumer tastes, expressed in terms of monetary demand, will change in accordance with changes in economic, political and social conditions.

As a manager, the businessman has also to do the choosing. Management itself is a purely economic function. Its task is to cut out waste wherever it exists, which implies making the maximum use of a given quantity of resources. The manager is always striving to become, in economic terms, more efficient, so as to maximise his return both now and in the long run. Some of the methods he uses may outwardly have little or nothing to do with economics; indeed the most vital quality of management is leadership. Nevertheless most, if not all, of his decisions will involve economic judgments either directly or indirectly. To operate efficiently, the manager has to combine scarce means in such a way as to achieve minimum costs now, and to ensure that his costs will continue to be minimum in the future. He has to choose particular combinations of factors of production, which differ in both technical efficiency and price, and the nearer he comes to the economist's model of a rational use of resources, the more efficient will his operations be and the larger his return. He must also be certain that his operations are consistent with the economic, social and political conditions of the country in which he works. In the past, businessmen tended to disregard the long-term future, since most of their executive actions were mainly concerned with the short-term. On the other hand, the economist has devoted more time developing sound analytical tools for the long term than he has for the short term. This difference of emphasis between long and short term has also helped to divorce economic from business thought.

This does not mean that all businessmen should be economists or that all economists should become businessmen. Indeed, each has his own field of operation, and each has his own contribution to make. But the point to be made at the outset is that economics is not just an adjunct to business; it is a vital sector of it. For this reason alone, the academic economist should devote more time to the development of theoretical models for short-term analysis, which could help the businessman in understanding more clearly the mechanics of the market. And this can be done only by a much closer co-operation between the economist and the businessman.

There are three particular fields in business where economics can be of direct help. They are not mutually exclusive, and a considerable area of overlap exists between them. But they are sufficiently different in essential form to permit demarcation for purpose of discussion. The first field might be termed the *economic radar system* which can forewarn the business about changes in the economic climate both as regards countries and commodities. The second field is as a *tool of management*—the use of economic principles as the basis for management information and of statistical techniques as a means of processing the measured information, to help businessmen to take the right decisions and subsequently to check the rightness of those decisions. The third field covers *business strategy* or the long-term outlook—the assessment of the economic future of those countries in which the firm has invested (either fixed assets or goodwill), of those industries in which the firm is interested, and of the relative position of the firm within its industry or industries.

Economic Radar

All firms are affected in one way or another by changes in general economic conditions. Some firms are concerned primarily with international conditions, others with national conditions, while others still with regional conditions. But all are interested in two particular aspects; (*a*) the change in conditions as they affect the

economic well-being of a country or an area, and (*b*) the change in conditions as they affect commodities. The economic climate of a country and its stage of economic development will influence the demand for different products and also the various ways of combining factors of production to achieve minimum cost. Changes in commodity markets will influence a firm's demand for materials and its search for possible substitutes.

In following the economic fortunes of a country or a commodity market the businessman has to enter the conventional realms of economics. This aspect of applied economic analysis has been well developed in recent years, despite the fact that there is no single theory of applied economics in which the business economist can seek refuge. The national income approach helps to put the vital changes into perspective, and with its aid, any person with an intimate understanding of how an economy works can foresee likely changes in economic conditions some months ahead. Political, social, psychological and even sociological factors have often to be taken into account as well, which makes the process of appreciation much more an art than a science.

The basic tool is the construction of a composite model based on changes in the supply and use of resources in real terms, on changes in the flow and use of funds (money), and on changes in overseas transactions. The analysis of resources shows the real changes in activity that are likely to occur during the coming period. The flow of funds reveals the monetary factors at work, from which it is possible to estimate the inflationary or deflationary potential and how prices will be affected. In fact, data are not available to do this properly, so that the monetary and financial equations have to be worked out another way. The balance of payments analysis shows how the changes in supply and use of resources in invisible items and in the terms of trade will affect Britain's overseas trading position. To this must be added an analysis of the operations in the sterling area as a whole and a study of international influences, so that the balance on the overseas account can be translated into changes in the central and dollar reserves.

The details of this type of analysis are well known. The problem, from industry's point of view, is to be able to do such analysis each quarter for, say, twelve months ahead, and to do it within a month after the end of a quarter. The results can only be of value to the businessman if they provide adequate warning of likely changes in the economic climate, and this consideration makes speed important. Assessing the past and present is necessary only to enable a forecast to be made of the future. The vital thing in plotting the course of economic development is not assessing the absolute amount of resources, etc., but the rates of flow and changes in the rates of flow, and it is often far easier to quantify one's 'feel' of such changes rather than guess an absolute quantity. Moreover, changes in rates of flow give far longer warning of the actual changes that are likely to occur in the economy, so that it is possible to detect phenomena which may not become noticeable for many months ahead.

In creating this model, no item—and certainly not stocks—must be regarded, statistically, as a residual. Each must be independently assessed, and the equality of the two sides of each account is achieved in the same way as fitting together pieces of a jig-saw. The dove-tailing together of the three component models to form an integrated one is done by the process of iteration, to ensure that both direct and indirect effects have been allowed for—where they are significant.

For certain firms the conclusions or forecast drawn from the national analysis may have to be broken down for particular regions. For others, whose interests are world-wide, the quarterly analysis will have to be done for all the major countries and even perhaps for a number of minor ones too, so as to build up a world picture.

The second task of the economic radar system is to forewarn about changes in the commodity markets, not only in a general economic sense, but also for the

specific purpose of helping the firm in its normal buying operations. Price movements are conditioned by changes and expected changes in supply and demand. The problem therefore is to estimate in terms of quantities the likely real changes and then to assess the expectations of change. The technique is essentially the same as that used in the resources table—the compilation of a supply and demand table—to show whether the pressure on the market is likely to be bullish or bearish. It can be refined to give a good indication of likely price movements during the coming season. Here, too, absolute quantities are not so vital as an assessment of the degree of change. Indeed, to achieve complete accuracy would be a hindrance rather than a help for it would mask the proper understanding of the concepts and the interrealtionships—in particular the translation from *ex post*, to *ex ante*, to a future *ex post* —which, from the point of view of analysis, are far more important in determining future trends.

The economic appraisals of the main commodity markets can also provide a check on the economic appraisals of countries, particularly for those countries where commodities form the major source of income, while the fact that these analyses are done independently provides a further check on the personal judgments inherent in the various assumptions that have to be made.

This type of economic analysis can make a positive and direct contribution to the taking of a decision, by informing the manager of the likely economic conditions (political events permitting) in both countries and commodity markets over, say, the next twelve months. It also makes an indirect contribution by reducing the influence of the psychological factors, which can play a very important part in price formation, since to be forewarned is to be forearmed. Moreover, this general analysis of countries and commodities helps materially in the two other fields of economic application, namely as a *tool of management* and in *business strategy*. It not only provides measured information for calculations involving estimates of the future; it is also the basis for long-term projection, since one cannot predict the future with any reasonable degree of certainty without first knowing where one is now.

Tool for Management

It is in the field of management that economics has so far made its least effective contribution to business, although it is here that it can probably help most in the drive towards efficiency. Its failing has not been because of any lack of theory, but rather because economics was hitherto unable to provide simple management information. It is indeed in the economics of the firm, that the barrier between the economics faculty and the business community has been most felt. The economist has developed sound principles, but until recently they could not be put into a realistic form, so as to guide businessmen to maximise their profit.

The accounting profession, on the other hand, fashioned a simple structure of management for the businessman, but, for the sake of simplicity, it had to ignore changes in both absolute and relative prices. The exclusion of the effects of alternative choices converted business problems from economic to technical ones. This limitation of conventional accounting data is fully realised by many accountants, but unfortunately not by many businessmen.

The problem is to construct an information system that has not the limitations of conventional accounts, so that, given sound personal judgment, right decisions can be taken. The system should in fact provide the *minimum* of correct information; quantity can be reduced and significance retained or increased by proper processing. No manufacturer would dream of selling raw materials to the housewife and expect her to do her own processing. Yet in the field of management, managers have long been given masses of unprocessed and undigested information and have been expected to operate efficiently.

The correctness of the measured information depends on the concepts on which it is based rather than in the precision of the data. If the information is not to be misleading it must be based on sound business principles. The basic concept is that of cost, which as economists we know to be nothing more than the foregone alternative use. Once a purchase is made the record of the actual price paid is of interest only to the economic historian and to the auditors who have to ensure that the published accounts comply with the provisions of the existing Company Law. It is the relative current cost rather than the actual price paid that is significant for the business decision, yet the conventional system of accounting uses only actual price paid. The ubiquitous influence of historical cost accounts can be seen in official U.K. statistics, where it takes from six to twelve months for lower import prices to be reflected in lower retail prices, given constant conventional profit margins.

The firm must be able to value correctly not only its wealth but also its income in order to be able to assess risk. Otherwise it might reject some opportunities that should be accepted, or alternatively it might accept some that should be rejected. This means that the replacement cost system must be used for the profit-and-loss account as well as for the balance sheet. Two methods are open to a firm. The first is to continue to use historical costing in the basic accounting system and to convert the data to replacement cost at every level where the information is necessary for the taking of decisions. This conversion can be done quickly and cheaply by using suitable index numbers and simple mathematical formulae. The alternative is to introduce a system of replacement cost accounting. This is difficult because a unique system has not yet been fully developed.

Income is the net revenue from any operation after ensuring that the venture at the end of the period is in the same position as it was at the beginning of the period, that is, that capital both fixed and working, is intact in terms of current, absolute and relative prices. Once income can be truly measured, it is possible to calculate profits so that they can indicate changes in business efficiency and how those changes arose.[1] The true measurement of income also helps in determining the minimum price at which a product can be sold under competitive conditions, and in testing decisions about investment projects. It also provides a means of estimating the generation of finance during the particular period of operation.

All business decisions are concerned with the future, and the manager is only interested in the present and the past so as to help him to foresee the future better. The information he really needs for most of his operational decisions is:

(*a*) His current costs (i.e. in current prices)
(*b*) What his costs might have been, had he taken different decisions in the past, and
(*c*) What his costs will be as the result of his present and future decisions.

And yet it is precisely these three items of information that the conventional accounts are not designed to provide. They can provide the first only if the profit-and-loss account is based on true replacement cost. They contain no technical apparatus at all to provide the second, while the third can be provided only as a simple mechanical extrapolation of the past—the dangers of such a technique (particularly when based on historical costs) are all too obvious.

With the aid of economic principles and statistical techniques, it is possible to design management information so that it can readily fit into a simple model, which can reveal the likely effects of the decisions. In this way managers could test the effect of their decisions even before taking them; there would still be an element of risk, and the size of the risk would depend on the reliability of the assumptions.

[1] See *Incorporated Statistician*, Vol. 8, No. 1 (October 1957).

This model would be equivalent to a business radar system and capable of forewarning managers of the likely results of their actions so that, if necessary, they can make the appropriate alterations. It would be wrong to imply that businessmen have hitherto taken decisions blindly. The need for such information has always been felt, but in the past it has been the preserve of the back of the envelope or the blotter. The aim of this new system of information is to reclaim this territory that conventional accounting has not been able to handle and to systematise it by providing an adequate frame of reference.

Admittedly it will remove many illusions and make complacency about decision-taking impossible. The manager would be constantly made aware of exactly where he stood—and of all the dangers and opportunities that are open to him. Ignorance is certainly bliss, but the function of the new system of management information is to reduce the area of ignorance. Excessive safety margins be revealed for what they are, and 'muddling through' would no longer be a respectable technique. Although the manager would lose the shelter of the conventional rules-of-thumb (which for most decisions are highly misleading), more of his decisions will be right. And, further, he would be able to delegate a number of decisions that, in the past, he felt he must take himself.

It is because correct management information removes the illusions, that the need for simplifying techniques in processing information becomes all the greater. Some processing of information is necessary in any case, in order to reduce the quantity and to increase its significance. Proper processing can also help to train managers and enable them to act more efficiently. It is not possible within the scope of this paper to discuss all the techniques available to business. Indeed, not all the techniques have been tried out to see whether their theoretical possibilities live up to practical application. One or two examples are given here to demonstrate the possibility both of reducing guessing and of reducing quantity.

Risk-taking is the essence of business, and businessmen pride themselves on taking a 'calculated risk' without realising that risk can be truly calculated.[1] The phrase has become a cliché for good management, and rightly so, in so far as it means that uncertainty is reduced by improving the assessment of a particular event occurring. But whereas the businessman's 'calculation' of risk is done by instinct developed through experience, the economist's or statistician's calculation of risk is done by mathematics based on experience methodically accumulated and scientifically assessed.

The calculation of risk is itself a risk, since it depends on the probable recurrence of past data. Indeed its limiting assumption is that conditions of the future will be similar to those of the past, or at least that allowance can be made for the effects of any expected changes. This could be a significant limitation, were it not for the fact that the likelihood of differing conditions can also be assessed. Nevertheless, it restricts the use of this technique to those business decisions that have to be taken frequently and which can be regarded as routine ones. In the first place adequate past data are likely to be available for them, and in the second the limiting assumption about future conditions is unlikely to be serious, since the length of time involved between decision and fulfilment is generally relatively short.

A typical business problem where *probability analysis* can provide far better information to the manager than what he has hitherto had, is that of buying. This technique can assess much more accurately than the normal run of guessing how supplies are likely to arrive and how the level of stocks is likely to fluctuate. In this way it facilitates the businessman (if he so desires) to profit from fluctuations in supply and prices, since he could anticipate expected price rises by building

[1] The difference between a business guess and a statistical estimate is simply that a business guess has a 50–50 chance of being right, whereas the probability of the statistical estimate being wrong is 0·5.

up stocks. The anticipation of likely price falls is more difficult, as the businessman has to assess how long he can stop buying before the risk of running out of stock outweighs the advantage of buying at lower prices in the future. This particular risk can also be calculated statistically, but the calculation is very complex and often can be done only by using what is known as the *Monte Carlo technique*, which reconstructs statistically the patterns of past experience against which can be assessed the likely consequences of any proposed action. It can thus calculate the risk of running out of supplies incurred in holding any given level of stock, which when related to the cost of stockholding enables the manager to decide much more precisely at which level of stock he is willing to operate. But stochastic analysis can also be used for many other business problems, especially in factory or production planning, such as the allocation of work to a given number of machines, or alternatively in decisions about the correct number of machines for a given amount of work, or again in constructing models to help in the understanding of marketing operations, and particularly of the effects of advertising campaigns. Indeed this technique, if properly employed, could help to revolutionise management information.

Those business decisions that are taken infrequently and sporadically cannot be aided by probability analysis, even though such decisions are generally more important (in terms of money involved per decision) than the routine ones. They cover many if not most of the decisions about capital projects, new products and such-like. Here the businessman must use his 'hunch'. But he can still be helped by the provision of proper economic and market forecasts. The decision information should not only reveal the various courses open to the manager and the likely costs and results, but also the vital relationships involved, so as to provide a better perspective. In this way the manager would be able to use his 'hunch' more intelligently.

The *theory of games* is a technique that could possibly be developed to help the manager in taking these 'unique' decisions. Its help would be not in assessing any particular project *in vacuo*, but in revealing the best possible action against likely competitive reaction. In particular it can help the manager in formulating the best wrong decision—the yardstick that can be used as a standard of minimum performance. The theoretical structure of this technique is well known, but there is considerable difficulty in applying it, particularly in drawing up the so-called 'pay-off' matrix. Even if the theory cannot be used to give a precise and determinate answer, it can be extremely helpful in training managers to handle some of the aspects of a competitive situation. If a computer could be developed specifically for business purposes and so combine the reliability and precision of the digital computer with the flexibility and easy manipulation of the analogue computer, the theory of games could be developed as an aid in undertsanding many of the requirements of business strategy and tactics. It could help, for example, to determine what tactics should be adopted in launching and selling a new product in a market with, say, one exceedingly powerful rival and the threat at various levels of other potential rivals. It could also help to indicate the course that should be followed in the short run to maximise profits and also to demonstrate what action would maximise profits in the long run. Further, it could show how far these two aims are in conflict. This understanding of the mechanics of the business operation is vital in the taking of right decisions, because no economist or statistician can ever hope to evaluate the business problem to such a point that concrete and determinate answers are always available.

Linear programming is already fast becoming an established technique in business in solving the best choice for a given objective from among a number of possibilities. When it is used for a problem that in the past has been solved by traditional rule-of-thumb methods, the reduction in cost is generally quite small, of the order

of a ½–1 per cent. It has, however, much greater success in the planning of new operations, and here it is far superior to those rules which have been evolved empirically. Its disadvantages is that in its application, simplifying assumptions about prices have to be used and they are making the technique too rigid. The development of *dynamic programming* (using forecasts of future prices within a stochastic model) should be of greater help to business. It is rather ironic that a mathematical technique designed primarily to help economic theory should be used in practice in such a way that the economic problem has to be regarded almost as a technical one. For those particular problems, where in the short run the economic problem does approximate to the technical one, linear programming can clearly be used to advantage. But such business problems are relatively rare, except perhaps under monopolistic conditions. Its main field of application at the moment is in the production process, but it could also be used to great advantage in helping to decide which product or products in a multi-product firm should be expanded in order to minimise profits. Such an application would obviate the need for arbitrary allocation of overheads, while at the same time assuring that profits after allowing for overheads were maximised.

The second group of techniques consists of those to reduce the quantity of information needed to take the right decisions and subsequently to check them. '*Figureless accounts*',[1] for example, can be used to obtain very quick and simple statisticial information about changes, and it is changes and rates of change that are more important to businessmen than absolute figures. It can be used to great advantage for those decisions where speed of information is more important than precision, and its main area of application is in the provision of brief, speedy routine data for purposes of management control.

For particular problems, where a small margin of error is more important than speed, an *ad hoc* investigation would be necessary, and this can be facilitated by using *sampling techniques*. The latter can also be used in connection with accounting information, not merely to assess the precision of data but to provide a cheap and quick system of book-keeping and to reduce the quantity of information needed for control purposes. To use sampling techniques in this way, it is first necessary to analyse the part that statistical information plays in decision-taking. Many managers insist on extremely precise information about the past in the belief, or hope, that it will compensate for their lack of knowledge about the future. This analysis of decision-taking emphasises the irrelevance of much of the present management information in those decisions which occur seldom and sporadically.

Index numbers and trend analysis are other techniques that can reduce the quantity of management information. Both can be used to advantage either individually or together for purposes of management control. At present they are more generally used as additional information, instead of replacing a lot of the unnecessary detailed figures.

In this field of management information, the economist and statistician should work closely with the accountant. Indeed, the actual application of the techniques might well be the accountant's task with the economist co-operating only to ensure that the correct concepts and the appropriate techniques are used to achieve the agreed objectives.

Business Strategy

There are four particular aspects of business strategy that have to be considered, and each is derived from the assumption that the firm is to live for ever. If any organisation is to continue as a profitable enterprise, it must be as efficient as its competitors and more efficient than its potential ones. But it must also ensure that its own

[1] See *Incorporated Statistician*, Vol. 8, No. 3 (May 1958).

economic objective is complimentary to and not competitive with the economic welfare of the country or countries in which it is operating. It is only when the manager's own operations help the national economy to achieve long-term equilibrium (economic, social and political), that he can be certain that his own end is not in conflict with the national end. Hence he must have a clear picture of the long-term economic development of those countries in which the firm is engaged.

In this sphere the businessman has been and still is well served by the academic economist. The tools for long-term analysis are adequate, and the renewed interest in the theory of economic development will undoubtedly help to show which assumptions are most appropriate under particular economic, social and political conditions. It is therefore possible to provide a realistic view of the likely economic future of certainly all the major countries and also of most of the minor ones.

But for business, the economic picture in twenty or fifty years' time is not enough. The businessman does not only want to know what lies at the end of the road, he also wants a good idea of the road itself—he wants to be forewarned of the conditions to be experienced in getting to the end of the road.

No economy is static. In some periods of history the rate of change may be very rapid, in others very slow. If a business is to remain profitable, it must be able to adapt itself to these changes as they occur, and at a speed that does not put it at too great a disadvantage compared with its competitors. Clearly all firms cannot be first in the field, but the efficient business should be able to estimate the cost of not being first and even the cost of being first, since the firm that adapts most quickly to change will not necessarily make most profits—other things being equal.

The businessman must be able to relate each stage of economic development to likely changes in patterns of wants and demand, to likely changes in production and distribution techniques, to likely changes in types of materials and, of course, to likely changes in the supply of labour, capital and other factors of production. This more detailed analysis, which is missing from current studies of economic development, has to be supplied by the business economist.

Business itself determines the pace of economic development, but it must operate in an environment that is conducive to expansion. In a country blessed with efficient and competing firms, where businessmen are psychologically and fiscally encouraged to take bold decisions, the rise in the material standard of living is likely to be much greater than in a country where the competitive spirit is restrained by controls, by public opinion, or by political ideologies. The government's rightful task in all this is to ensure an economic, social and political atmosphere that will encourage businessmen to take risk and to strive always to become more efficient.

A useful starting point for this more detailed analysis is Thorsten Veblen's thesis [1] that although people generally like to hunt with the pack, they prefer to be with the leaders rather than with the trailers. Man has an innate desire to be respected by his fellow-beings, and the outward form of commanding respect is, in Veblen's terms, 'conspicuous waste' and 'conspicuous leisure'. This is the psychological and social mainspring of the acquisitive motive. In economic terms, it implies that as the people's standard of living rises, they desire to buy goods that outwardly show their relative 'opulence' and that help to give them more leisure. And statistical analysis of consumption trends in the major industrial countries over the past fifty years confirms this thesis.

In this way it is possible to translate growths in the standard of living into increased demand for particular product groups and types of products. This analysis can be supplemented by a study of *per caput* consumption of different products related to *per caput* national income. This second step can be very

[1] *Theory of the Leisure Class.*

illuminating, since tastes and expenditure patterns of those countries with a higher material standard of living tend to be transmitted—with certain time lags—to those with a lower one. The translation of tastes is not exact. Differing national characteristics and social institutions as well as psychological, social, technical and political developments all play their part in moulding them into an acceptable form. Nevertheless this analysis can give a good idea of likely changes in the pattern of consumer expenditure and demand over time, and if firms had been able to use it fifty years ago, they would not have been misled as to the type of products whose demand was likely to expand. But this analysis is not able to indicate with any degree of precision the timings of events, and this is where the businessman's acumen again comes into its own. He has to determine the tactical timing on his 'hunch', because it can be just as unprofitable to be too soon into the field as too late.

The third aspect of business strategy is the assessment of the firm's future relative position in its industry. Although all goods are competitive to any firm's products, some are much more competitive than others, and it is these that are of particular interest. At present the businessman has to rely on his intuitive skill. Some have been and still are extremely successful at it, and no amount of economic research could improve their judgments. Others are not so fortunate, and it is for them that the theoretical development in studies of relative shares of market, relative profitability, capital-to-output ratios and so forth could be of great assistance.

The problem is essentially one of assessing relative managerial efficiencies. If all firms published proper profit-and-loss accounts based on economic concepts from which could be determined how far changes in profits reflected changes in efficiency and how and where these changes occurred, the assessment would be simple. Unfortunately, such published accounts do not exist. The problem therefore has to be solved in a round-about way.

The significant factors to be considered are: (*a*) an assessment of the management team—is it intelligent, capable and alert? (*b*) the business philosophy of the firm; (*c*) the technical know-how (in its widest sense) and the techniques available in and to the firm. Such qualitative appreciation could be used in forming the quantitative assessment of competitors' operations. The theory of games could also help here, despite the fact that the decisive factors defy quantification. It can at least reveal the vital relationships.

The short-cut method of assuming constant share of market in estimating future sales facilities quantitative analysis but begs the whole question, since it obviates the need to study closely the business make-up of one's competitors. Such a study, if it is done intelligently, is certain to be profitable no matter how nebulous the result may be.

The fourth aspect is strategic planning. It is impossible to say at what point strategy takes over from tactics; nor indeed is it necessary or even helpful to have a clear-cut division of the two. Business tactics is akin to a game of chess, where not only each move and each counter-move have to be considered, but also a whole host of subsequent moves and counter-moves. Strategy being longer term is concerned more with policies, but the distinction is not rigid except at the two extremes. Strategic planning covers marketing considerations, research projects and capital investment schemes. In each sphere, the problem is to evolve a policy that will help to maximise profits despite competitive reaction, and one which at the same time contributes to the long-term national equilibrium. Here judgement plays a greater part than technique, which can help only by putting the relevant factors into a proper perspective to give managers a better understanding of the operation.

But to plan strategy properly, a firm must also know what new opportunities in both products and countries are likely to be available, and such information is

readily available from the long-term economic analysis. In addition, a firm must be able to estimate its long-term needs of skilled labour, management, materials and finance. This information could be obtained from the mathematical models designed for decision-taking, as they can show the effect of expansion and rate of expansion on such requirements. These estimates can then be related to forecasts of future supplies, not only of the type of factors of production currently used, but also of likely substitutes that are expected to be available in the future.

In this way long-term marketing plans can be dovetailed into long-term supply plans, long-term production plans, personnel development plans and long-term finance plans. None of these estimates must be regarded as blue-prints. They are no more than indicators displaying the likely future, but they are none the less valuable for that.

Teamsmanship

In business economics, as in all applied economics, personal judgment plays a vital part. It comes into play in relating and sifting data, in assessing its relevancy to a particular problem, in accounting for all these intangible factors that have to be considered, but which defy even the first order of quantitative analysis, and in the appreciation and interpretation. But the business economist has to go even further. His judgment, like the businessman's, must be attuned to the business problem. It is the function of the manager to make the decision—to decide on what risk in relation to what cost. But unless the economist has complete sympathy with and a full understanding of the manager's task, not only in a general sense but also in the particular circumstances in which the manager is operating, his contribution to the efficient operation of a business is bound to be limited. This means that the economist should be brought into and made part of the central management team, so that he can be acquainted with the same facts and nuances as the managers. If he is forced to stand on the periphery, he is liable to give only half advice, which is worse than no advice, since few people are capable of specifying a problem with sufficient precision of thought to enable an 'outside' adviser to function properly. Nevertheless, even as a member of the central management team, the economist must not get immersed in the day-to-day details of operations, otherwise he is likely to lose his perspective.

The fact that judgment plays an important part in economic advice implies first that the calibre of the person must be high both as an economist and as a member of the management team, and secondly that economists will work better in a team than individually.

The value of the team method cannot be exaggerated, particularly for economics where a qualitative problem has to be translated into quantitative terms. The steeling of wits, the breadth of discussion and the variety of approach, all contribute to provide a better answer more quickly. Moreover, the personal judgment of each is tempered by that of others. Those firms fortunate enough to have a sufficiently large group of economists to enable them to be divided into teams to cover different aspects of the work, can develop this method further by ensuring that the various fields have a limited but definite overlap. Provided duplication is avoided by grouping the smaller teams within a bigger one, no waste of resources is involved; instead a further check is provided on personal judgment. The inevitable variations of views based on personal assessments can be reconciled within the group, so that the conclusions can be presented in an unambiguous form and without confusion.

In a small firm teamwork in the field of economics is impossible. Indeed one man might well have to cover all the various economic tasks and certain other functions as well. Or, it may even be that some of the economic tasks may have to

be sacrificed for other functions, which to that particular firm with its limited resources seem more urgent.

Epilogue

The advent of the European Common Market and the Free Trade Area will mean an increase in both the scope and intensity of competition, and British industry already realises the need for efficient management, if it is to hold its own, much less expand. The interest currently being taken in operational research, in cybernetics, and in the use of electronic data processing is very encouraging. But the new aids to management, particularly the use of stochastic analysis to reduce guessing in business decisions, have still to be properly developed. For this, industry needs a supply of really first-rate economists and statisticians—and unfortunately that supply is lacking.

It may well be that the usual three-year economics course is no longer sufficient to provide an adequate groundwork for graduates, and British universities may be forced to follow the example of their Continental brethren by providing much longer courses or by promoting it to a second degree. But the problem goes deeper. Apparently economics, and the social sciences generally, have lost their attraction of the 1930s, and the really bright students are no longer presenting themselves to these faculties. If this is true, then it is indeed a very serious handicap, and the universities must set about the task of providing sufficient intellectual stimulus and visions of practical application to catch the imagination of the brilliant sixth-formers, and in this way compete with the attractions of nuclear science and chemical engineering.

There is scope enough. Short-term analysis, micro-economic analysis and the link between micro- and macro-analysis all require urgent development. Even the theory of value has not been developed to the point where it can be readily applied by the businessman, particularly as regards price elasticity in the short term and long term. There is also need of a comprehensive study of the propensity to save under different economic and social conditions.

Again, considerable scope for economic research exists among the archives of firms. Historical studies of changing patterns in consumer wants and expenditure gleaned from sales records could provide valuable information for the businessman. The results of motivation research could also be profitably used in determining and analysing the forces that condition consumer wants and consumer demands. Then there is the refinement of the production model—to give but a few instances of possible new areas of intellectual stimulus. The development of economic studies along these lines would have the further advantage of having a practical bent, which would make students realise the opportunities of applying such a live subject to the affairs of business. After all, economics was never intended to be just an intellectual exercise.

Admittedly, this regeneration of economics will involve a wide use of mathematical treatment, since in no other way can the vital relationships be revealed in a proper perspective. Moreover, quantitative analysis is essential for economic work, where every 'feel' must be translated into figures to prevent misunderstanding. This is not a plea for econometrics, which unfortunatley has failed to give the stimulus to the development of economic thought that was originally expected. It is certainly not the dynamic approach that econometricians have claimed it to be. If anything econometrics has tended to make economic thinking both rigid and static (despite the fact that its models can handle a number of variables), because it has regarded quantification as an end rather than a means. Some of the statistical and mathematical models used by business economists are very elegant and provide neat results, but they do not lead the business economist astray into thinking that

precision and neatness are in fact possible or even necessary. The figures represent nothing more than quantitative indicators; the important thing is the logical structure of the model.

Applied economics will always depend more on good brainpower, ample common sense and a deep understanding of sound economic principles, than on any mechanistic device, whether electronic or otherwise. It is the academic economist's task to develop the sound principles, so that the business economist can apply them with the aid of the various mathematical and statistical tools that have been developed, to help British business to become even more efficient.

THE DIRECTORY OF NATURAL HISTORY AND FIELD STUDY SOCIETIES

THE Council of the British Association for the Advancement of Science have, at the invitation and with the financial backing of the Carnegie United Kingdom Trust, prepared a new edition of *The Directory of Natural History Societies* which was first published in 1948 by the Amateur Entomologists Society.

The Directory, which was published at the beginning of March, 1959, covers archaeological, astronomical, biological, botanical, geographical, geological, meteorological and zoological societies, both national and local amateur bodies—and other cognate societies. The information given for each society is listed under the following headings: title and address, name and address of Secretaries, aims and objects, conditions of membership, amenities, meetings, publications and affiliations. Societies are arranged alphabetically under the main headings of natural history, archaeology, archaeology and natural history, societies with general interests, geology, astronomy, and meteorology. The arrangement of the societies within the sections is strictly alphabetical according to the titles supplied by the Secretaries. There is a final geographical reference section in which the names of the societies are listed alphabetically according to counties. This geographical section has been compiled so that naturalists moving from one district to another may be able to get into touch with others having similar interests.

The Council of the Association hopes that the publication of the *Directory* which contains information about some 800 societies will stimulate field studies throughout the country and, by increasing the possibility of co-operation between professional and amateur scientists, will make a real contribution towards our understanding and appreciation of the world in which we live.

Copies of the *Directory* may be ordered (20*s*., plus packing and postage 1*s*. 6*d*.) from The Secretary, British Association for the Advancement of Science, 18 Adam Street, Adelphi, W.C.2. A limited edition of interleaved copies is available at £1 5*s*., plus packing and postage 1*s*. 6*d*.

VISUAL AIDS AND THE PRESENTATION OF SCIENCE

I. SCIENTIFIC FILMS AT HARROGATE

THE BRITISH ASSOCIATION awarded two Diplomas of Merit at the recent Harrogate Festival of Films in the Service of Industry (April 21 to 25); one to 'Between the Tides' (British Transport Films. Sound. Colour. 22 minutes.) for the film which provides the best presentation of science to the public; and the second to 'Schlieren' (Shell Film Unit. Sound. Colour. 15 minutes.) for the film which provides the best exposition of the scientific principles underlying an industrial process.

These films were excellent examples of the kind of scientific material which is available from industrial organisations.

'Between the Tides' is a detailed study of marine life on the Pembrokeshire coast and in beautiful colour photography provides close-up studies of for example, whelks, limpets, periwinkles, sandhoppers, sea snails, crab, lobster, starfish, prawns, sunstars, blennies, sea squirts, lump suckers, moluscs, razor shells, tube worms, pike fish, sea lemon, sea slug, sea hare, conger eel, orrweeds, anemones, algae, pennywort, barnacles, sea campion, sea pink, gulls, kittiwakes, herring gulls, blackback, razorbill, puffins, and gannets.

'Schlieren' was made as the result of the interest in this photographic technique aroused by an earlier Shell film 'High Speed Flight, Part I—Approaching the Speed of Sound'. This system, much used in aerodynamic research, was developed in Germany in the nineteenth century. It uses the refraction of light caused by variations in air pressure and temperature to record photographically phenomena which are normally invisible. A high intensity light source is focused via a condenser on to a concave mirror and passes a light beam through pure glass panels in a wind tunnel. This beam is brought to a focus from another concave mirror. At the focal point the narrow centre panel of a three-coloured light filter is placed. In the examples shown in the film the filter has a green centre panel and on either side one broad red and one broad blue panel. When a wing section is placed in the tunnel changes in pressure deflect the light beam which then passes through one or other of the outer coloured sections of the filter. Changes in pressure are gradual and are recorded as areas of colour, whilst shock waves are sudden changes which are recorded as strong lines of colour. Thus a cine-camera can record in this instance a black silhouette of an aerofoil against a green background with the varying areas of air pressure shown in blue or red. In its popular exposition 'Schlieren' gives examples of this type of aerodynamic research—shock waves above and below a wing section; aileron 'buzz'; air-flow around a model aircraft at twice the speed of sound; airflow around a single wedge, useful for hypersonic aircraft; aircraft affected by nozzle design, useful in rocket research. But the film also shows many homely examples useful perhaps for physics teaching—air rising from a human hand; cold air circulating round an icecream; warm air from a wall radiator; an inverted bottle emitting a colourless heavy gas; the mixing of two colourless liquids; the flow of sugar solution in water; even the shock wave induced by a motor car sparking plug; and the air emerging from the vent holes of a recorder on which is being played 'Pop goes the Weasel'.

Both these films are examples of what enlightened industrial film sponsors especially can achieve. Quite correctly, and yet oddly in scientific context, 'Between the Tides' was entered in the sales promotion category at Harrogate. It is effective sales promotion, too, because you must be a very insensitive member of the

audience not to have the urge to go to the seaside and explore for yourself. If you have this urge then the film has already sold you the idea of travelling by some road or other to the sea.

At this Festival 112 British films (selected from more than twice that number of entries) competed for awards in nine categories which included: public relations; sales promotion; education and training; health and safety; productivity and efficiency; and human relations and welfare. Many of these films are of technical and scientific interest and readers of *The Advancement of Science* who are interested in them should obtain a copy of the programme from the Organising Secretary, Richard T. Edom, 3 Portman Chambers, 7–9 Baker Street, London, W.1.

II. REVIEWS

1. *Television and the Child*

By Hilde T. Himmelweit, A. N. Oppenheim and Pamela Vince

Published by Oxford University Press. Price 42*s*.

This survey of the effect of television on the young was published in 1958 and was widely reviewed both in the national daily and weekly press, and in specialist journals.

The terms of reference of the research group were to study the impact of television on children and young people. In 1954 3,000,000 homes only had television. A total of 4,500 children were tested in London, Portsmouth, Sunderland and Bristol and from these 1,854 matched viewers and controls were selected for detailed study and analysis. With the opening of a television transmitter at Norwich the possibility arose of making a 'before and after' study and the survey questionnaires were given to 10–11- and 13–14-year-old children at a time when hardly any family in Norwich owned a television set. The survey was repeated after a year comparing groups with and without television and 370 cases of viewers and controls were investigated.

Two main techniques were used for the survey. Children were asked to keep diaries filled in privately each day at school for one week. Six weeks later they were given a questionnaire. Measures of their personality were obtained as well as ratings by the teachers of their behaviour and personality characteristics. In addition eleven special studies were made of groups not covered by the main survey—for example, mothers' records of their children's viewing habits; opinion studies among teachers and parents; studies of the effect of television on school performance, etc.

Some of the principal findings of the survey were as follows:—

(*a*) More time is spent by children and young people on television than on any other leisure activity (11–13 hours' viewing weekly).
(*b*) The single most important background factor influencing interest in and time spent on viewing was intelligence.
(*c*) Viewing rapidly becomes a habit and even 'veteran' viewers only reduce their viewing by two hours weekly compared with new viewers.
(*d*) From the age of 10 at least half the children watched adult programmes and three-quarters of their favourite programmes were in this category.
(*e*) Their tastes in television reflected their tastes in other mass media.
(*f*) The more choice of programmes available to the child the less likely he is to come into contact with programmes which, from an educational viewpoint, are most worthwhile.

(*g*) The values of television can make their impact best if they are consistently presented in dramatic form and if they touch on ideas or values for which the child is emotionally ready.

This very comprehensive report—over 400 pages of text and 100 pages of appendices—because it is so full of detailed and sometimes nice distinctions tends to lose much of its impact on the non-specialist reader. It is to be hoped that at some future date some of the detailed case studies made during the survey will be published as a short volume, since from these one will be able to judge better the impact of television on the individual child and to relate this to the broad statistical picture as a whole.

2. *Your Film Show*

Issued free by the British Electrical Development Association, 2, Savoy Hill, London, W.C.2.

This is a most useful guide for intending film users who have no experience of organising film shows. So often one sees films marred by bad projection, inadequate blackout, poor acoustics, or faults in the layout of the room and a simple document of this kind should play a useful part in improving standards. Copies may be obtained free from the address indicated above.

3. Wall Charts on Atomic Energy

Title: 'Atomic Energy'. Size: 30″ × 20″ Price: Set of five—25*s*. Single 5*s*.

Obtainable from Educational Productions Ltd., 17 Denbigh Street, S.W.1.

Latest addition to Educational Productions series of wall charts is a series of five sheets devoted to nuclear energy dealing respectively with the atom, radio-activity, nuclear fission, nuclear fusion and atomic power. With bold clear diagrams and well spaced print they give a useful picture of the main technical facts and in the case of the last sheet (which provides details of the power reactors at Calder Hall and Dounreay) they relate theoretical knowledge to its industrial application.

THE WORK OF A BILATERAL TECHNICAL AND MODERN SECONDARY SCHOOL IN A NEW TOWN[1]

by

G. B. C. PALMER

Headmaster of Mark Hall Secondary (Technical and Modern) School, Harlow New Town.

THE establishment of a new secondary school in a new town appears to give a clear example of the challenge and demands made by society on education in a technological age. For a new town is itself the physical result of planned and co-ordinated technology applied to human settlement; its industries tend to be those that have shown recent technological advance—electronics, plastics, radio, television, radar equipment, various forms of research—work demanding large amounts of highly specialised training applied to a small physical product. And its society, with its extremely unusual age structure—(of 42,000 people, 40 per cent are under the age of 15 and 20 per cent under the age of 5), shows a marked homogeneity; although there are houses of various types, shapes and sizes, there are no socially distinct zones or areas. All mothers shop at the same serve-yourself shops, attend the same community centre whist-drives; the very great majority of men drive or cycle along the same road and cycle track to and from the factories in the same industrial areas. Thousands of families certainly buy their thousands of perambulators from the same shops on the same hire-purchase terms, and the thousands of children all go to the same county schools.

The prime demands made on the secondary schools in a society such as this appear to be two: first, to send out entrants to industry and commerce and agriculture able to enter those activities at all levels, whether unskilled or semi-skilled at 15, craft apprentices or equivalent at 16, student apprentices and entrants to sandwich courses and colleges of technology at 18; and, as an addendum to that, to send out people with that attitude of mind which recognises that secondary schools are only the second stage, and that they must continue their education and training to a further degree. The second demand does not apply to man's work, but to his larger life; and that is to give him or her a personal vitality, vision and interest, so that his emotional, spiritual and intellectual horizons shall not be bounded by the increasingly marked physical homogeneity apparent around him. It is true that the town planners have judiciously and cleverly, by grouping and subtle changes of design, given some variety to the physical habitat. But very much more is demanded, an ever-widening mental horizon.

If, as I think, a new town is but a hardening crystallisation of those general tendencies apparent in society today, our consideration of what one school is attempting to do to produce young men and women to enter their work at many varying levels—and at the same time to widen their larger horizons—may perhaps help us to bring into focus the problems and opportunities today facing education in a technological age.

When, ten years ago, the Essex Education Committee decided on the pattern which secondary education in Harlow New Town was to take, it both abandoned

[1] Paper delivered to Section L (Education) on Tuesday, September 2, 1958, at the Glasgow Meeting of the British Association.

a tripartite organisation, and avoided the comprehensive school: instead, its policy was to build six bi-lateral schools, each for approximately 1,000 boys and girls. The first of these, Mark Hall Technical and Modern, was opened four years ago. Her sister school, a Grammar/Modern, was opened a year later, and others have followed in turn, Technical/Modern, and Grammar/Modern, at approximately yearly intervals. The fourth of the six schools will open very soon. In this way it was the policy to minimise difficulties caused by segregation following the General Admission Examination (the so-called 11-plus), to facilitate transfers and movement between selective and non-selective streams in the same school and between different schools, and to avoid the difficulties of the mammoth school. At the same time, by pursuing its policy that the minimum standards for admission to the selective streams, both Grammar and Technical, shall be the same, by provision of buildings and equipment of equal munificence, and by a system of inter-locking boards of Governors, the Committee has gone far towards establishing 'parity of esteem'. Harlow New Town was chosen for the experiment, or as I would prefer, for this 'pioneer work', because the education of old Harlow and the surrounding district was as yet 'unre-organised', there being in the area five or six all-standard village schools, but no secondary schools,—in particular there was no established County Grammar School with a known 'ethos' or 'kudos' with which a new bi-lateral might have found itself in competition. It was further decided that each school should be of six-form entry—of which two forms should be 'selective' (Grammar or Technical as the case might be) and four non-selective.

That was the policy. And the Committee then proceeded to appoint its headmasters, and to let them get on with the job. This they may have been able to do with remarkable independence, untrammelled by advice. Indeed, both the Committee and the Ministry have shown commendable diffidence—almost reluctance to legislate or interfere with what is going on. Now what, in fact, has actually happened? What is the practice?

The first obvious comment of course is that children do not get themselves born in tidy packets of six forms each year, of whom one-third are 'selective'. In fact, Mark Hall in some years has admitted ten forms—but with the opening of more secondary schools, the position seems to be stabilising itself at eight forms each year. Of these, two forms are made up of children who on the results of the General Admission Examination and their records in the primary school are adjudged suitable for selective education. What determines whether or not a child shall go to either Grammar or Technical streams of the two schools? In practice, it is very largely the choice of the parents which is the determining factor. It is interesting to note that in the early days, such is the prestige, or glamour, of the name 'Grammar' that an overwhelming majority of parents opted for the Grammar stream—of the Grammar/Modern School—but within one year this has been substantially changed so that the Technical Streams have an approximately equal share of the more able entrants.

In general then, a year's entry at the age of 11 contains children of all ranges of ability, the few very bright potential university scholars, the large number of those of moderate and mediocre ability, and, at the other end, those who only with difficulty can read and write. In each year, those of lowest ability are grouped together—sixteen or eighteen of them—in a group which officially is known as Remedial, but which staff-room wits know as irremedial. The non-selective forms are graded roughly according to their general ability. Each form overlaps in ability so that, as we should expect, children in the top of the highest non-selective form are every bit as good as some of those in the selective form. Because of this overlapping, we find that it is not necessary to make much movement from form to form. In particular, the Committee's selection technique appears in fact to select the right children and although it is the prerogative of the headmaster to promote

children from non-selective to selective forms if he considers such promotion justified, in fact, only two or three such promotions are made each year in each school. The two selective and the highest secondary modern form first pursue a two-year course which differs very little from that of their brothers and sisters in the Grammar stream. English is the essential core subject, and there are also Mathematics, Science, History, Geography, French, P.T. and Games, Art, Crafts, Music, R.I. In common with all the other forms of the school, this curriculum continues in the third year with an increased amount of time given to practical subjects.

There is no line of segregation as between Technical and Modern children—they come in at the same door, hang up their hats and coats together, eat side by side, play together. They are all in the same teams, the same choirs, the same Gilbert and Sullivan show. Indeed, often in school they work together, as in practical subjects they are 'settled' together, and in the gym and on the field the timetable is so arranged that a selective form, and a non-selective form, together form a class or group for instruction or play. Staff, too, teach both modern and technical forms, so that a master's English lesson with the brightest class will be followed by one with the far from bright.

The normal course for the selective children is for five years (end of the year in which the 16th birthday falls), and for the others the course is of three years and a term; or two terms, or four years (end of the term in which the 15th birthday falls). We have, however, pursued a very interesting development in Essex—the 'extended course', by which a child at the end of the third year opts to stay on for two further years—a voluntary raising of the school leaving age. About 50 per cent of our secondary modern children in Harlow now do this—and not always the very bright ones.

The particular 'growing point' of this bi-lateral school—and this is the whole point—is that in their fourth and fifth years these children are now 'setted' for *all* their subjects with the selective children,—so that the somewhat arbitrary line drawn by the 'eleven plus' examination is completely eliminated. At the top of the school in the fourth and fifth years there are four or five 'sets', some at G.C.E. standard and some at a lower standard, in each of the core subjects, and two or three sets in the alternative subjects offered. In this way, not only can we give an almost indefinite series of options of subjects but those subjects themselves may be taken at different levels, a three-dimensional choice. The potential scientist pursues a course of English, Mathematics, Physics, and Chemistry, with History, Geography and Art, or Wood or Metalwork; the potential craft apprentice takes English, Mathematics, General Science, Metalwork, and Technical Drawing, and the potential bank employee takes English, Accounts, French, Geography, and Commerce, with Domestic Science and Needlework; the girl going into commercial work will take English, Accounts, Shorthand and Typewriting. In all this, all boys and girls take at least one practical or craft subject, and most take two.

This system of 'setting' and options commences in the fourth year. The individual's place in the pattern is arrived at during the last term of the third year, after consideration of his school work, a personal interview, and perhaps a meeting with his parents also. His place can, of course, be varied at any subsequent time right up to the last term. You will notice that the child does not make a decision —Engineering *or* Commerce *or* Building—it is far too early for that, but he has the opportunity for some choice or emphasis for the subjects he wishes to stress.

Until next year, we shall not be able to see how our selective children measure up to the criteria of an external examination. The school has not been open long enough. Already, however, because they have entered the school at all ages from 11 to 15, we have extended-course secondary modern people taking the G.C.E.—thirty-four last year and sixty this year. The number of successful candidates in

each subject, expressed as a percentage of the total number taking that subject, is just about the same as the figures for the whole country. This year the average number of subjects taken by the sixty has been between four and five—and these are chosen from twelve different subjects.

It is our policy to retain at school a sixth-form course for those who are able to profit. Sometimes parents are more hopeful of their child's ability than we are. And it is very difficult to discourage, as we in fact have done in some cases, children for whom advanced work is beyond their powers. Nevertheless, in September, our first sixth form will commence, eight children, with advanced courses in Physics, Chemistry, Biology, Mathematics, History, Geography and Art—extremely expensive from the point of view of staffing in the early days. These have all come from secondary modern forms—from 1959 onwards we shall of course recruit greater numbers from our selective fifth forms.

So far I have said little of the term *Technical*. There are two points I would make—the first is that one does not make the course technical merely by adding technical drawing or shorthand and typing to the curriculum. It is rather more profound and more subtle than that. It is rather the question of approach, of emphasis, of method and illustration: how to present Mathematics, Science, History, and Geography in real direct terms without losing sight of the essential grammar of the subject.

The second is the obvious point, but needing re-iteration nevertheless. That success in business and industry—for which in the past the more vocationally minded technical schools specifically trained—depends less and less on early training in trade practice, which we can leave to industry, but more and more on a sound general training and an appreciation of first principles, and a habit of application to work. Paradoxically, a technological age is requiring schools to be less 'technical' in the narrow sense, but more attentive to first principles and sound learning.

So, then, children are going out from this school, at all levels. We must admit, that of those who leave at 15, some are of very low attainment, can just read and write, and are suitable only for unskilled and semi-skilled work. Others are leaving at 15 or 16, with no G.C.E. qualification, the postmen, the gardeners, the farm workers, and van and lorry drivers, and their sweethearts and wives, the thousand and one providers and maintainers of all the services of modern society. At 16, too, come the craft apprentices for industry, workers in banks, insurances, commerce, housing, estate management, laboratory technicians, policemen, tradesmen. At 18 there is the student with two or three A-level subjects, to pursue a further course at University, or College of Technology, or to enter industry full-time or by sandwich course; here, too, we find, the nurse, the physiotherapist, the teacher.

These all, in an increasingly homogeneous society, must have their interests, in school work and out of school, stimulated in many varied and variegated ways—to play games rather than to read the results, to move about, camp, cycle, canoe—to enjoy and to be critical of what they read and hear and see.

But it is just those people who most need this widening, who find these things the most difficult. I refer to the least intelligent, the semi-skilled and unskilled. Technological advance surely makes even the provision of employment for these people increasingly difficult. If a new town is typical of the changes in society brought about in the age of science, then the incipient unemployment of the dull and backward may be a menacing omen of which society must take note, at its peril. These, if idle in numbers, are potentially dangerous.

The school is set in wide gracious fields, an exciting, invigorating and stimulating building—with equipment and facilities for manifold interests, with staff of very varied background and experience. All these factors help us to try to see that our children leave us equipped to commence work *at all levels*—and at the same

time with spirits and minds open to look beyond the physical homogeneity facing them.

Time is of course yet too young for us to evaluate fully the achievements of the school: what are four years? But it is perhaps a happy augury that our prestige is increasing: parents of children due to enter in two or three years' time are already pressing me to put their names on a waiting list. Dr. Lawrence, our Chief Education Officer, has told me that he considers that any valid judgment cannot be made until the school has been open for at least ten years. But we already look forward to that judgment with some measure of confidence.

DELINQUENCY

SECTION J (Psychology) devoted the morning of Tuesday, September 2, 1958, at the Glasgow Meeting of the British Association, to a discussion on 'Delinquency'. Professor T. Ferguson, C.B.E., Professor of Public Health in the University of Glasgow, opened the symposium with a paper entitled 'The young delinquent in his social setting'. Dr. D. H. Stott, Lecturer in Psychology in the University of Glasgow, followed with a paper on 'Boys on probation: maladjusted or normal?' Mr. L. T. Wilkins, a Research Officer employed by the Home Office, then presented a paper on 'Patterns of post-war juvenile and young adult crime'. The last of the invited contributions to the symposium was given by Dr. R. G. Andry, Senior Clinical Psychologist at St. Thomas's Hospital, under the title of 'Paternal and maternal roles and delinquency'.

The account of the symposium which follows has been prepared for publication by Dr. D. H. Stott.

The study of delinquency is no less than the study of human behaviour as a whole. Since in the latter we are more at the foothills than at the peak of a science, we cannot claim to be able to explain all the phenomena of delinquency; on the other hand, it cannot be said that, because many of its problems remain obstinately unsolved, we have not made substantial progress in identifying causes.

It all depends, of course, upon what we mean by causes, and how far back we are prepared to go. Ultimately the causes of delinquency are biological, in the ecological sense—the suitability of man to his surroundings, and of his surroundings to man. At a less generalised stage the causes are seen in sociological terms—the greater or lesser likelihood of delinquency occurring in one social setting or another. Finally, when we come to inquire why certain social and familial environments are conducive to delinquency, we have to try to understand the motivation of individuals, and this brings us within the field of psychology.

Professor Ferguson opened the symposium with a summary of the principal findings of a study, which he carried out with Dr. Cunnison, of the 1,349 Glasgow boys who left school in January 1947. Their approach was a sociological albeit eclectic one: from as many quarters as possible they gathered their information about ability, temperament, school attendance, job preference, home background, and they achieved the feat of following the careers of these boys over a period of years. Their study afforded an interesting example of the statistical drag-net technique: you do not know what is at the bottom of the pond, but you decide to pull it up just the same.

What they brought up, it must be conceded, would be of great value to the infant making up its mind to come into the world. We are hereby warned by Professor Ferguson's study to exercise some discretion, in our next transmigration—first and foremost about our family, if we wish to avoid becoming delinquents. For those of us already in middle age a summary of other *do's* and *don't's* might prove valuable. The basic statistic is that 12 per cent of all boys between eight and eighteen years of age were convicted at least once. Our first *don't* is not to live in a slum, or our chance of conviction will go up to 22 per cent. Our second is not to be a dunce, or, to put it scientifically, not to fall within the lowest two centiles of scholastic ability, for if we do our chances of legally offending will be 26 per cent. Professor Ferguson exercised the usual scientist's reticence as to whether these dunces were such because of poor hereditary ability, because they had not the temperament or character for learning, or because they were born into families where learning is

unimportant and perhaps discouraged. Nor were we told whether the educational failure was one of the causes of the delinquency, or whether both it and delinquency happened to be characteristics of the same pattern of culture. Indeed, Professor Ferguson never allowed himself to be caught into using the word 'cause'. The nearest he got to it was to refer to 'the importance of low scholastic ability as a factor contributing to delinquency'. By 'contributing', we gathered he meant 'contributing to the statistical result', but it was difficult to be quite sure. In any case, we have been warned that if we do come at the bottom of the class we are likely to be associated, statistically or otherwise, with delinquency.

Our third is to insist firmly that we shall not be born into an overcrowded home, for in such a home our chances of waywardness, if we are also backward, soar to 44 per cent. Our fourth will be more in our own hands: we must not be caught, again if backward, in a stop-gap job three years after leaving school, or else our conviction ratio will touch 40 per cent.

Our fifth will be a matter of inches: if we fail to top 57 of them by our fourteenth year, our moral future will be dimmed, even if we have landed in a good home. Here Professor Ferguson, no giant himself, waxed sentimental, and with a tell-tale displacement-activity shifted from the Sciences to the Arts. Bill Sikes, he reminded us, needed the services of an undersized boy for squeezing through small windows.

How much of all this was new and how much expected? The answer is, probably, that, at least for Glasgow, Professor Ferguson has provided us with some basic dimensions. The bugbear was, as always, that the discovery of a statistical association as a self-sufficient research activity still leaves us guessing as to the nature of the underlying relationships or—to use that good old-fashioned word—causes. Professor Ferguson put forward no hypotheses to be confirmed or otherwise; and he drew no conclusions. One got a feeling that beneath the associations with slumdom, overcrowded homes, low scholastic ability, dead-end jobs, we might arrive at a conceptual model of a way of life of which delinquency was part and parcel. But of such we were given no hint.

However, two unexpected associations might well provoke us to further thought. Why should undersized boys be more prone to delinquency even though they come from 'good' homes? Adlerian compensation for physical inferiority is rather dated as a theory, for physically subnormal and handicapped boys do not normally seek self-expression in delinquency. The Gluecks, working with Sheldon's categories, found linkages between physique and delinquency-proneness, but it was the athletic and physically fit in their case who were more common among the delinquents.

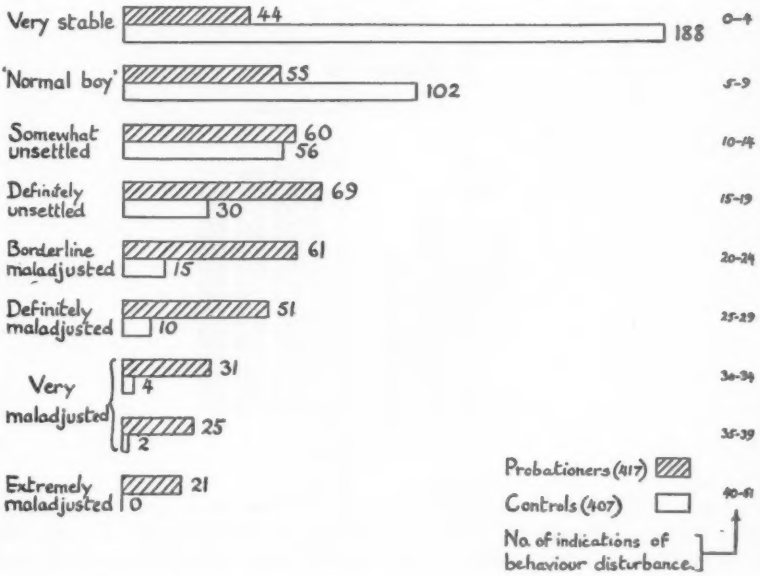
The second revelation was that a relatively high proportion of boys educated in special schools for the mentally subnormal became delinquent for the first time after leaving. Perhaps it was just that their whole development was delayed, but against this their delinquency-rate was high even during their school-years. At work, surprisingly, these subnormal boys had done reasonably well; it was their leisure, apparently, that they were unable to manage. There seems here to be a case for provision of club or other facilities by which the backward school-leaver can be helped to adapt himself socially.

In his paper, 'Boys on probation: maladjusted or normal?' Dr. D. H. Stott, who has for long been the target of the sociologists, reported upon his recent study of the 1957 year-group of Glasgow probationers. Its aim had been to obtain evidence upon which to judge the claim of at least one school of sociologists that delinquency, far from being a symptom of maladjustment, is in some neighbourhoods ('sub-cultures') a sign of good adjustment, a conformity to local expectations about the growing-up of a normal boy.

Dr. Stott conceded the intensely local distribution of delinquency, and showed a map which demonstrated that Glasgow, along with other big cities, had its 'black'

and its 'white' areas. But he pointed to the experience of teachers that the typical delinquent was by no means a popular, emotionally thriving, well-adjusted boy; he is generally an unhappy outcast shunned by all other children except the few of his own type. Nevertheless, each theory may be in a measure true, and account for a proportion of delinquents. The test would be the latter's non-delinquent behaviour. The research used the Bristol Social Adjustment Guides as a means of obtaining a wide sounding of the behaviour, such as was observable by teachers, of the probationers and of controls nearest in age in the same schools, This schedule, Dr. Stott pointed out, does not ask the teacher's *opinion* about a child or rely upon an interpretation of character in terms of subjective 'traits'. The teacher merely had to mark, by underlining, objectively observable acts or instinctive expressions of mood, such as eagerness or resentment in stated concrete situations, so that there was little scope for 'halo' effect. Thus the behavioural data were obtained in their

TABLE I



raw state, without the observers' knowing their diagnostic significance. The records were coded, by placing transparent sheets over them, into the standard attitudes—stable, unsettled or maladjusted—recognized by this diagnostic instrument. For the purpose of this survey the total number of unsettled and maladjusted items registered by each boy was used as the index of behaviour disturbance.

The differences in the 'scores' for the probationers and for the controls were very striking, as can be seen in Table I. A modicum of unsettledness or bad behaviour at certain stages of his growth was allowed to the 'ordinary' boy, so that all those with nine or fewer 'bad marks' were considered within the normal range. This covered 72 per cent of the controls, but only 24 per cent of the probationers.

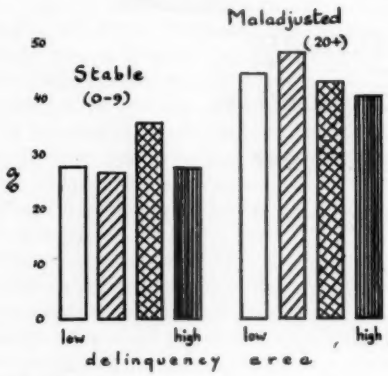
Thus about one-quarter of the delinquents might have been such for purely sociological reasons—ordinary boys for whom, in their neighbourhoods, a phase of law-breaking is part of the process of growing up. But from them would have to be subtracted those boys who turned to delinquency as a relief or escape from emotionally distressing family circumstances—without being rendered demon-

strably unsettled or maladjusted thereby. At any rate, it seemed that of boys put on probation in Glasgow, not more than this minority of one-quarter are likely to have become delinquent by way of adjustment to their sub-culture.

Probably the majority of boys faced with a sudden crisis in their family-lives would become 'unsettled' rather than maladjusted, and this group accounted for 31 per cent of the delinquents. Twenty-one per cent of the controls were 'unsettled', but the 'total score' method of classification hid the fact that the unsettledness of each of the groups tended to be of different types. The really big differences emerged from the score of twenty or over, this being taken as the 'maladjusted' mark. The histogram of Table I shows only 7½ per cent of the non-delinquents to be such, compared with 45 per cent of the probationers.

It remained next to reconcile this finding with the undoubted fact—upon which sociologists base their 'normal-boy' theories—that juvenile crime tends to be concentrated within certain neighbourhoods and among sections of the population of similar 'culture pattern'. Of these sociological interpretations the most favoured is

TABLE II

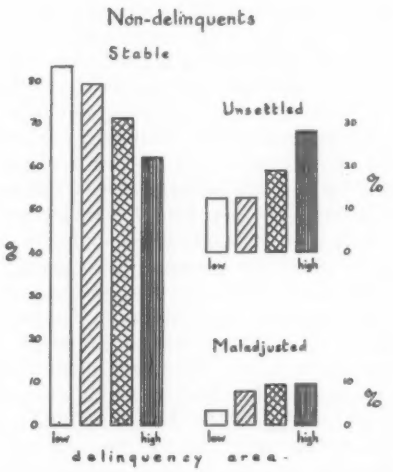


that of the 'delinquent sub-culture' already referred to, namely, that in some neighbourhoods delinquency is the 'done thing'—a characteristic of good adjustment rather than of maladjustment. If this theory were true of Glasgow, one would expect the high-delinquency spots to account for most of the above 24 per cent of well-adjusted probationers, while the maladjusted ones would be distributed over the somewhat better areas. To test this, the city was divided into five 'cultural' grades according to the prevalence of probation cases over the past three years. This was done on the basis of the percentage in each school, which had the advantage of ensuring religious (and to a large degree ethnic) homogeneity. (In fact, only four categories are shown in the Tables, because one group had no probationers at all, and hence also no controls.) Surprisingly, the result showed no discernible tendency for the probationers of any cultural group to be more or less maladjusted than any other. Table II shows 27·8 per cent of them in the lightest-delinquency schools to be of stable personality, compared with 27·4 per cent in the highest. Of the maladjusted, the proportions were 44 and 40 per cent. It was thus not true of Glasgow that within certain neighbourhoods delinquency is part of the process of growing-up for the normal, well-adjusted boy. This finding has a certain general validity, since of all cities Glasgow would probably be regarded as the most likely place to find delinquent sub-cultures.

The psychological finding of a close association between delinquency and maladjustment still needs to be reconciled with the unequal neighbourhood-distribution. One explanation would be that localities and sub-cultures which contain much delinquency also contain much maladjustment; in other words, the same social conditions or pattern of life are responsible for both. If this hypothesis were correct, we would expect to find that the non-delinquent controls in these unfavourable environments would also show a higher degree of maladjustment. Table III shows that this was indeed the case. In contrast to the random percentages among the probationers (Table II), we see here completely consistent trends: the proportion of well-adjusted boys falls regularly in the four grades of school, from 84 per cent in those of lightest delinquency, to 62 per cent in the highest; conversely, the 'maladjusted' rate rises from 3·6 to 9·5 per cent and the 'unsettled' rate from 12 to 28 per cent.

The conclusion was drawn that our basic problem is not one of delinquent propensities as such in certain neighbourhoods, but of 'black spots' from the point of view of mental health in general. The higher delinquency rates in them can be

TABLE III



regarded as symptomatic of a greater amount of maladjustment. It happens, however, that delinquency is the symptom which gets put on record and which attracts public attention.

But why, it may be asked, this concentration of the maladjustment-delinquency disease in certain localities? Dr. Stott suggested that the sociological picture of the delinquent-neighbourhood reflected a vicious circle of personal and social failure on the part of a small section of our city populations. Those who succumb to stresses and strains make worse stresses and strains for themselves, their neighbours and their families. The key-point is the breakdown in family-stability—reflected in much-advertised juvenile delinquency and glossed-over juvenile maladjustment. Under social and economic pressures those who have become submerged by their difficulties gravitate to the least desirable quarters; hence the concentrations of the socially unfit which appear as 'delinquent-subcultures'. This conclusion does not contradict so much as explain the findings of the sociologists. It has been the latter's attempts to explain the phenomena along purely sociological

lines—without the minuter sort of observation that entailed psychology—which have failed to stand up to the test of facts.

At the conclusion of this paper *Mr. John Mack* entered the lists on behalf of the sociologists in the best tradition of scientific dialectic. The maladjustment which Dr. Stott had detected, he averred, was itself a cultural phenomenon. Different patterns of culture had different ideas as to what was good and bad behaviour, and two-thirds of the items on the schedule used merely reflected this cultural factor. Moreover, children from an alien pattern of culture such as that of large sections of manual workers would be expected to react against the 'educational' pattern of culture that they had forced upon them in school.

Replying, Dr. Stott pointed out that the Bristol Social Adjustment Guides had been validated upon samples of predominantly working-class children, and that the standards of adjustment were by no means those of an educated élite. Furthermore, the items of behaviour were chosen so as to reflect basic instinctive reactions, rather like the tail-wagging of dogs. Such behaviour would be independent of the cultural differences found in European civilization; cheerfulness, moodiness, bad temper, loyalty to playmates, maliciousness, and so on were everywhere recognised and regarded in the same light. As for the supposed hostility generated by cross-cultural conflict, he instanced gipsy, fairground and bargee children; they adjusted themselves to a culturally foreign school-environment with remarkable ease and good nature, even sometimes making an effort to learn to read out of sheer courtesy to the teacher. Teachers in slum schools do not find themselves confronted with any general hostility for cultural reasons, for the stable child of any cultural group can quickly adjust himself to a strange social environment. Finally, and most tell-tale of all, the maladjustment of the delinquents showed itself more towards other children—of their own culture-pattern—than towards teachers.

The discussion wound up with the affirmation that the sociological and psychological approaches to delinquency are complementary rather than competitive. Each studies different stages of causation. The sociologist examines behaviour and ways of life and thought which are common to a group, and which may make delinquency more or less likely within it. The psychologist tries to follow these influences through to the responses and emotions of individuals.

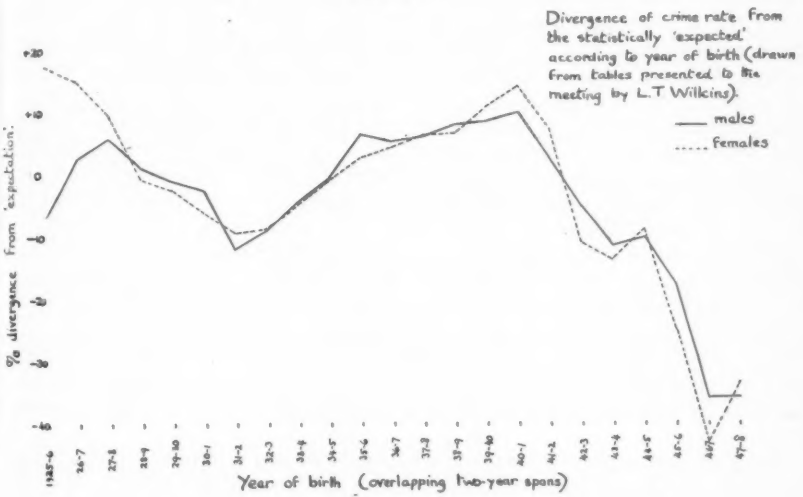
The next speaker, Mr. L. T. Wilkins, served a feast of statistics fresh from the new research department of the Home Office. On the sheets he circulated there were 1,330 of them, and we whipped over one pageful after another as he unfolded his story. He had set out to investigate the intriguing riddle of why juvenile delinquency totals should go in 'vintages'. There are good years and bad years (good and bad being how you take it), the totals fluctuating with uncanny consistency throughout the cities of Britain. After two or three years of steady falls, such as 1953–4, when those in authority sit back with sighs of relief, and staffs of approved schools and Borstals begin to get anxious about their jobs, there comes along a bumper year and the newspapers become delinquency-minded again.

Mr. Wilkins showed that all this had something to do with the years in which the delinquents were born. Boys with birth-dates between 1935 and 1941 had a delinquency-rate consistently above expectancy. It was remarkable that the corresponding figures for girls followed an almost identical pattern. We have taken the editorial liberty of plotting the percentages above or below expectancy, as given by Mr. Wilkins, on the accompanying graph (Table IV). (It should be explained that since the data were drawn from records of convictions in which the ages of children were given in years, it was only possible to give the years of birth within two years. For example, a child who appeared in the records as aged 10 years in 1958 could have committed his offence on December 31 when he was 10 years and a day, or on January 1, when he was 10 years and 364 days, giving a range of two years.)

The extraordinary similarity of the graphs for each sex seemed to herald a significant discovery. But a discovery of what? Mr. Wilkins refused to be drawn. The statistician's job was to detect associations; he was never justified in making interpretations or drawing conclusions. A statistical result could be a rock upon which a pet theory could founder, but it is a single relationship, an isolated epistemological event inadequate for the erection of a conceptual structure. Mr. Wilkins, in effect, offered us a guessing game and then withdrew behind his figures, uninterested in their implications. We would be less than human if we did not rise to the bait.

It is hard to avoid the inference that the high delinquency-proneness of the 1935–1941 births had something to do with the war. Children born during these years would have been subject to evacuation; many of their fathers would have been away from home during several years of their early childhoods; their mothers may have been on war work and almost certainly harassed by problems of feeding and

TABLE IV



clothing; and finally there was the bombing. It is noteworthy that the summits of the curves for both sexes are reached for those born in 1940–41, the period of the blitz; and there is an interesting little subsidiary peak, again for both sexes, during 1944–5, the time of the V2's. This guess will have to be referred back to Mr. Wilkins, with a request for an analysis of the figures by areas bombed and not-bombed.

When we try to single out which precise aspects of the war-situation were chiefly responsible, we begin to see how wise Mr. Wilkins was not to commit himself. If we first take evacuation, very few children under five were evacuated without their mothers, and evacuation virtually collapsed after 1942; so this is evidently not the answer. If it was the absence of the fathers, or the general difficulties of wartime existence, this should have affected children born throughout the war and not just those born during the early part. The effects of the bombing, if any, must remain highly speculative. It might have been that owing to disturbed pregnancy the nervous constitutions of some children born during the blitz were affected; but this could not explain the beginning of the rise in 1935–6. It is hard to imagine that three- and four-year-olds would have been so frightened by events

they did not understand as to suffer traumatic effects. It seems evident that the neat patterning of the curves hides a concatenation of influences.

When we work back from the summarised divergencies from 'expectancy' to the annual ones for each year-group, there are further surprises in store. Children of the vulnerable birth-periods are by no means consistently so over the whole age-range of 8–20 years for which their delinquency incidences are given. For example, the higher rate among those boys born between 1935 and 1938 inclusive is accounted for almost entirely by their greatly increased delinquency-proneness during 1956 and to a lesser degree during 1955—when they were eighteen to twenty years old. Up till that age they had fluctuated in the main fairly close to the statistical 'expected'. It would be a bold theoretician who would postulate a wartime influence operating between the first and ninth years of life, which only manifested itself in delinquency during early manhood. It would be more reasonable to assume that the higher expectancy of delinquency for the 1935–9 birth-dates was due to some contemporary factor of the 1955–6 period and nothing to do with the war.

The detailed annual divergencies from 'expectancy' yield yet another surprise. For boys the peak delinquency-proneness, found among those born between 1938–9 and 1941–2, is almost entirely confined to the earliest years of their boyhoods (between eight and eleven or twelve years) during which they can be convicted in a court. After twelve years they are a little, but not much, above average. The same phenomenon is observed among girls, except that in the 1938–9 year-group the incidence is less regular; it holds good in the 1939–42 groups even more markedly than among the boys. This looks much more like a genuine war influence. And at this stage the reader should be reminded that the 1938–9 year-group included all children who reached a given age at any time during those two years, so that those who were most delinquency-prone could have been born, notwithstanding the '1938', after the war had begun. The hypotheses which seem most worth exploring are those relating to very early life and even pregnancy, especially with regard to the effects of bombing. But before we can get much further we shall need to have actual birth-dates of a large series of delinquents rather than rely upon official returns, which only tell us that each offender was so many years old at the date of his conviction. It would also be important to have the more precise figures by localities, so that a comparison can be made between heavily and lightly bombed areas.

So much for the guessing game. Each expert can be relied upon to play his hand according to the particular winning cards which he thinks he holds. As usual, every proffered interpretation tends to leave us in the air because the data were not collected in order to deal with just that possibility. Once again, also, we see the limits to the information which can be culled from eclectically devised official statistics (to which we must make an honourable exception of those of the Registrar-General for Scotland). But science demands that we go on guessing—with the proviso that we play the hand out to see who is right and who is wrong. No doubt Mr. Wilkins and his associates will do us the service of statistically debunking our guesses as we make them, even if they refuse to guess themselves.

Dr. Andry, the last contributor to the symposium, challenged the recent tendency to see delinquency mainly as the outcome of early deprivation of mother-love. In the research which he reported he set out to explore the relative importance of paternal and maternal remissness. Eighty recidivist delinquents, the same number of non-delinquents, thirty mothers and fathers of the first sort and the same of the second were subjected to a succession of cunningly devised questions. It is a measure of the softening up of our population by the random-sample pop-the-question brigade that only 2 per cent of delinquents' parents and 1 per cent of those of the non-delinquents jibbed. But as far as the fathers were concerned, Dr. Andry admitted to cornering them in public-houses.

His first significant finding was that the delinquents made comparatively little complaint about the affection they received from their mothers, but over half of them thought they ought to have had more from their fathers. As expected, most of the non-delinquents felt that both parents loved them equally.

Dr. Andry then subjected one or two clinical stereotypes to statistical analysis. A stock question of some clinicians is to ask the child which of his parents he thinks he resembles, on the theory that he will name the one with whom he unconsciously identifies himself. But almost exactly half the delinquents said they looked like their fathers and half their mothers, irrespective of which parent they felt closest to. Among the non-delinquents, again about half named each parent. The truth, it appeared, was simpler than the psychologists believed: about half of each group, for biological reasons, probably resembled each parent, and knew it. Perhaps we are not so much the victims of unconscious distortion after all!

His next questions aimed at the ability of parents to *show* affection. Few of the non-delinquents felt that either of their parents had difficulty in doing so—only 12 per cent of the mothers and 23 per cent of the fathers; compared with 52 per cent of the delinquents' mothers and 65 per cent of their fathers. Once again, the paternal deficiency is seen to be somewhat the more frequent—if, of course, the delinquents' reported feelings in the matter were accurate. It would probably be safest to regard these figures as confirming a tendency to uneasy or otherwise unsatisfactory relationships between the delinquents and their parents. Over half the delinquents, for their part, admitted that they felt themselves uneasy in showing affection for their parents. Considering that many of them would have found such an admission too painful, it is surprising that the reported percentage is so high; it almost certainly under-represents the prevalence of defective parent–child relationships in delinquents' families.

It then occurred to Dr. Andry to check the answers of the parents and their children against each other in order to see if the reports he was getting from each side about child–parent relationships were reliable. Contrary to the 'purist' school, who are apt to reject any personal statement as so loaded with subjectivity as to be scientifically valueless, it transpired that there was agreement between the statements from the two sides to the extent of 73 per cent among the delinquents and 87 per cent among the non-delinquents. One is reminded of William H. Whyte's dictum in *The Organization Man*: 'Someday someone is going to create a stir by proposing a radical new tool for the study of people. It will be called the face-value technique. It will be based on the premise that people often do what they do for the reasons they think they do.' Perhaps we psychologists often fail to see the conscious wood for the unconscious trees!

Returning to his main theme, Dr. Andry summarised his findings. First, the delinquents suffered to a greater extent from paternal than from maternal inadequacy of affection. Second, too much importance has been placed on the damage done by separation of the child from its mother in its early years. There was no significant statistical difference in this regard as between the delinquents and the non-delinquents. He finally concluded that some form of parental rejection over a period of years, rather than the much publicised early separation, was the chief causative factor in delinquency.

BOOKS RECEIVED

TITLE	AUTHOR	PUBLISHER	PRICE
Aspects of the Organic Chemistry of Sulphur	Frederick Challenger	Butterworths	40*s*
Nomenclature of Inorganic Chemistry (IUPAC)		Butterworths	15*s*.
The Annual Register of World Events	Ivison Macadam (Editor)	Longmans, Green & Co. Ltd.	£6. 6*s*.
Practical Metrology	K. J. Hume & G. H. Sharp	MacDonald & Co. (Publishers) Ltd.	10*s* 6*d*.
Elementary Matrix Algebra	Franz E. Hohn	The Macmillan Company	52*s* 6*d*.
Proceedings of Royal Society of New Zealand	Dr. J. T. Salmon (Editor)		
Transactions of the Royal Society of New Zealand	do.		
Slaves Unaware	Magnus Pyke	John Murray	16*s*
Chemistry Today	Arnold Allcott and H. S. Bolton	Oxford University Press	15*s*.
T. H. Huxley	Cyril Bibby	C. A. Watts & Co. Ltd.	25*s*.

CRYSTAL PHYSICS

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16s
15s.
25s.

SECTION A (Mathematics and Physics) and Section B (Chemistry) met together in joint session on the afternoon of Friday, August 29, 1958, at the Glasgow Meeting of the British Association, to consider a number of papers in the field of 'Crystal Physics'. Dame Kathleen Lonsdale, D.B.E., F.R.S., Professor of Chemistry at University College, London, in a paper entitled 'Man-made diamonds', discussed X-ray diffraction and spectrographic analysis of diamonds made in the U.S.A. which showed that they contained nickel inclusions so similar in structure to diamond that they may well have been the agent causing crystallisation to occur in the desired form. Dr. H. Judith Grenville-Wells, Department of Chemistry, University College, London, followed with a paper on 'Diamonds in the sky'. This paper indicated that diamonds are often found in nickel–iron meteorites, and that while early experimenters had felt that meteorites held the key to diamond synthesis their hopes were not realised. Nevertheless, a study of the Canyon Diablo meteorite played a part in the successful synthesis of diamonds in the U.S.A. Dr. I. M. Dawson and Dr. D. H. Watson, of the Department of Chemistry, University of Glasgow, then presented a paper entitled 'Molecules a million times larger than life', in which they considered the use of the electron microscope to study protein molecules and crystals. In general, the results of this study have shown that individual molecules are arranged in crystal lattice. The final contribution to the symposium came from Dr. J. W. Menter, Head of the Microstructures Group, Tube Investments Research Laboratory, Cambridge, and Dr. P. B. Hirsch, Assistant Director of Research at the Cavendish Laboratory, Cambridge. Their paper, which was entitled 'The imperfect crystal', showed that the lattice of atoms or molecules of which a crystal is made is usually disturbed by imperfections, and that the most important of these, dislocations, by their movement and interaction under stress largely determine the mechanical properties of the crystal. The paper went on to describe two approaches to the direct study of dislocations using the electron microscope. The Tube Investments Group (J. W. Menter, G. A. Bassett and D. W. Pashley) have obtained direct images of individual dislocations at very high magnifications where the crystal lattice itself is directly imaged and the lattice disturbance associated with the dislocation is seen. The Cavendish group (P. B. Hirsch, R. W. Horne and M. J. Whelan), working at lower magnifications, have obtained pictures of of dislocation arrays in metals and have studied their movement and interaction by cine techniques.

The paper by Dr. Dawson and Dr. Watson is printed in full below.

MOLECULES A MILLION TIMES LARGER THAN LIFE. THE DIRECT OBSERVATION OF PROTEIN CRYSTALS IN THE ELECTRON MICROSCOPE

By *Dr. I. M. Dawson and Dr. D. H. Watson*

As everyone knows, proteins are the basis of life. Human and animal tissues are largely composed of them, and, indeed, no life-process seems able to function without the contribution made by them. At a time when a great deal of biological research and of applied research is being directed to the way cells grow and multiply, it seems essential that we should have a clear understanding of the structure of the primary building bricks of which the cells are composed.

When I say that we do not know the structure of proteins, I do not mean that we have no information at all to go on. This is very far from being the case, since a

great deal of important work has already been done and a great deal of information has been collected. We are, however, still a very long way from knowing the position of every atom in a protein molecule and an impossibly long way from a discussion of the detailed variations in bond lengths within the molecules.

From Fisher's early work we know that the simplest and most elementary stage in the formation of proteins involves the formation of polypeptides by the coupling of amino-acids. For example, if we combine glycine with alanine we get the dipeptide glycylalanine:

$$NH_2.CH_2.COOH + HNH.CH(CH_3)COOH \rightarrow NH_2.CH_2CONH.CH_3)COOH + H_2O$$

This dipeptide, as it is called, can then add a third amino-acid, in the same way, to form a tripeptide, and the addition can then continue until a complex polypeptide is built up consisting of hundreds of amino-acid residues. With some thirty or forty amino-acids to choose from and the possibility of building polypeptide sequences from them in an infinite variety of ways, we can see that the polypeptides which result can be exceedingly complex. In a protein molecule which is formed from polypeptide units, we have the further complication that we may have several polypeptide chains, folded and cross-linked so that the properties of the complex may differ substantially from those of the component polypeptides themselves, due to the masking of some reactive groups in the folding process and the exposure of others.

It is this complexity and infinite capacity for variation which adapts the proteins as a class for the various requirements of the living cell. In this we can also begin to see some of the difficulties encountered in the structural study of these molecules, since they are very much more complicated than anything the chemist has tackled in the past. Since many structural variants are possible, even in proteins with the same amino-acid composition, it is a matter of some importance that the material studied should be pure and homogeneous. Synthetic proteins cannot yet be made, so that naturally-occurring proteins from carefully chosen sources must be isolated and purified. The aim is always to reduce the risk of contamination with molecules of a similar size and shape to a minimum. Although the usual criterion of purity, namely crystallinity, is not quite as valid as with simpler molecules, nevertheless it is a useful starting-point, and normally crystalline proteins which have been subjected to further purification by physical methods are used. The complexity of the molecules themselves means that any structural investigation is bound to be long and involved and it is rather important at the outset to choose the simplest molecules for study. If we can understand these, then a great deal of the information gained can be applied to proteins in general.

The attack on the structure of the group as a whole has been a two-pronged one. First, the original very crude approach of chemical degradation and study of the decomposition products has been developed into a method of considerable precision by Sanger and others. This work, which applies to the study of protein molecules in solution, was described by Dr. Sanger at the British Association Meeting a few years ago, and I need do no more than remind you of it at present. Secondly, there has been a full-scale attack on the structure of proteins in the solid state. For this, X-ray diffraction methods have been applied to single crystals of several proteins. If successful, this method has the advantage over the first method that it gives much more precise information about the arrangement of the component atoms in the molecule and the interrelation of the various constituents. It has, however, the disadvantage of being indirect. In X-ray diffraction studies we have, first of all, to guess the structure of the molecule, and then, by prolonged calculation based on the diffraction data, we can find out whether we have guessed correctly or not. If the imaginary structure is fairly close to the actual structure, then the calculations

will show in which direction we should proceed to arrive at the true structure. If, however, the assumed structure is incorrect, then the long period of calculation which follows will show this, and this alone. This method of attack works well for simple molecules, and with various refinements can deal with molecules of molecular weight up to 1,000 or so. Beyond this point, not only does the difficulty of guessing correctly become greatly increased, but the complexity of the calculation is so great that a very considerable amount of time and effort must go into each successive trial. For complex structures, there would be a very real advantage to be gained from a direct method of examining structures, without having to make any previous assumptions. To help to overcome some of these difficulties a direct method using the electron microscope is now being developed. The stage reached with this new method can be illustrated by the results obtained with the enzyme protein ribonuclease.

Everyone knows about the light microscope, since it is probably one of the most commonly used scientific tools. The electron microscope is a similar, but much more powerful instrument, which allows us to look at the structure of objects at very high magnifications. Roughly speaking, one can say that in order to see normal-sized molecules we require a microscope which gives the same increase in useful magnification over the light microscope as does the light microscope over the naked eye. The electron microscope has now been developed to a stage where it can in fact meet this requirement.

Before we consider photographs of ribonuclease crystals, it would be useful to have a more convenient yardstick than the centimetres and millimetres of everyday objects for measuring the very small sizes involved. To use a unit of size which is many million times larger than the objects under study is rather confusing, and for that reason the Ångstrom unit is used in this work. This unit was originally adopted for the study of molecular dimensions since it is of the same order of size as common interatomic dimensions. It is one ten millionth part of a millimetre, so that

$$1 \text{ mm} = 10{,}000{,}000 \text{ Å},$$

and with the light microscope we can see sizes down to 5,000 Å, and with the electron microscope down to 10 Å.

The ribonuclease crystals we examined were extremely small. At low magnification in the electron microscope they appear as shown in Figure 1, plate 1. This specimen has been shadow-cast so that the surface structure of the crystal, and, for that matter, the support is thrown into relief. From the lengths of the shadows we can, since we know the angle of shadow-casting, calculate the thickness of the crystal and also of any steps or projections on the crystal surface. This type of general view at comparatively low magnification is useful in that it provides a connecting-link between what we can see in the electron microscope and what we can see in the light microscope. The crystals are of the monoclinic form of the protein; the *b* axial length is parallel to the length of the crystal. The uppermost face is (100), so that the other axis is *c*. $a \sin \beta$ is perpendicular to the plane of the picture. Some structural detail is visible even at this low magnification on the crystal face and is in fact caused by successive layers of molecules on the crystal surface. These are incomplete molecular layers in the growing crystal. From the height of the edges of these steps we can obtain a fairly rough measurement of the molecular size in this direction. It works out to be around 30 Å and corresponds to the $a \sin \beta$ value found by X-rays.

When crystals lying on the support with various different faces uppermost are available, then this sort of technique can be used to gauge the size of the molecule from measurements of step heights.

This approach is, however, designed to show up the external form of the crystal,

and to see something of the structure of the molecule itself much higher performance in the electron microscope is required. Fortunately, with present-day microscopes we can see detail down to about the 5–10-Å level and, since we know from other methods of measurement that the ribonuclease molecule is about 40 Å long, this should be perfectly adequate for our purpose. For this high-resolution work, very thin crystal fragments were necessary. The reason for this was, of course, the obvious one that with more than a few molecules in line in the crystal, the picture would become rather obscure. However, various techniques have been

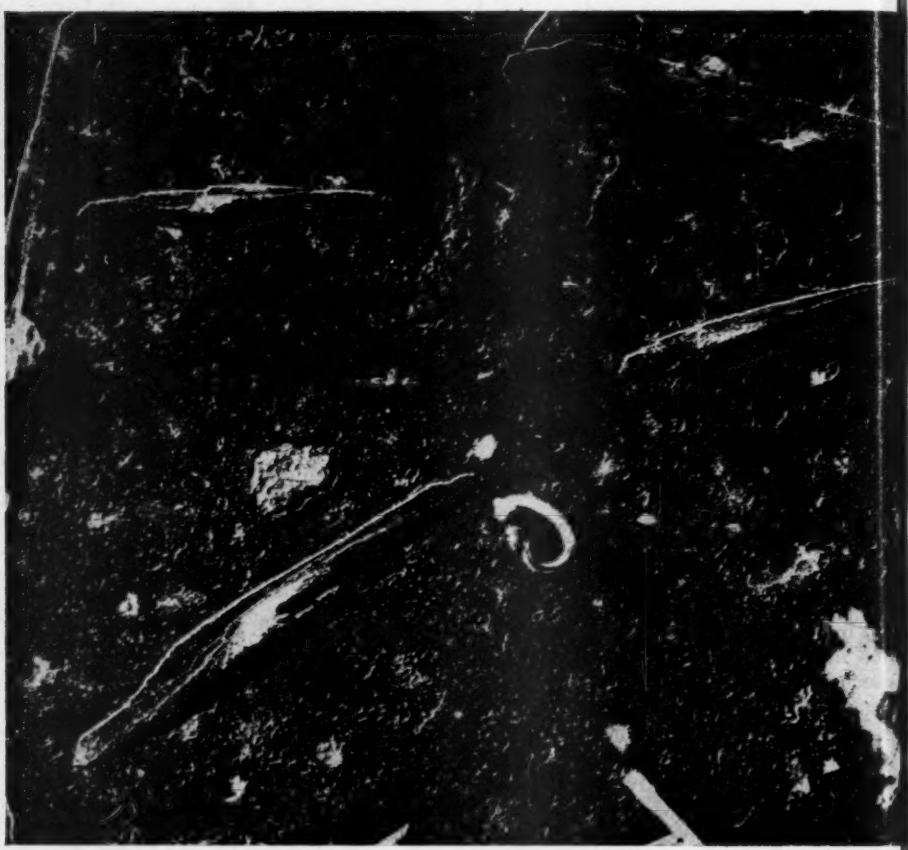

FIG. 1, PLATE 1

developed to produce very thin crystal fragments for direct examination in the microscope.

Figure 2, plate 2, shows such a fragment at a magnification of 135,000 times. In this case we are not interested in the surface structure of the specimen, so that shadow-casting is unnecessary, and we are interested rather in the structure which can be seen in the crystal fragment. As you can see, at this intermediate magnification the crystal has a striped appearance. When we look at it at much higher magnification in Figure 3, plate 2, this shows a considerable amount of detailed structure. The fine lines are spaced regularly over the whole area of the crystal fragment at an

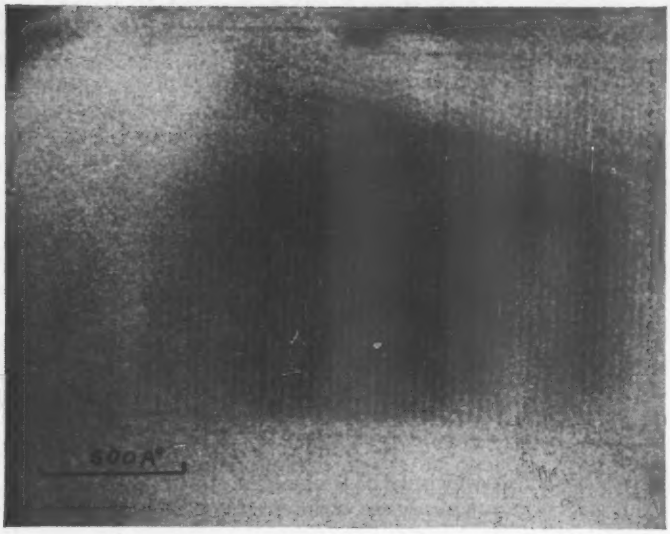

FIG. 2, PLATE 2

FIG. 3, PLATE 2

exact value fairly close to 18 Å. The coarse lines are much less regular in spacing, and values between 120 Å and 140Å have been found. The coarse lines are usually seen to be at a very small angle, about 2 degrees, to the fine lines. At one or two places we think that we can detect a separation of the 18-Å spacing into two 9-Å spacings in this picture, notably in the regions *A* and *B*.

We now have to consider what this banded structure means in terms of the structure of both crystal and molecule. Fortunately, from other, less-sharp pictures we can decide the direction of crystal axes in this picture. *c* is parallel to the direction of the bands, and *b* is perpendicular to the direction of the bands. There is obviously a very strong alignment of material parallel to the *c*-direction, with an intervening space containing relatively little scattering material. We have a kind of sandwich packing of the molecules in the crystal, with a suggested arrangement of

molecular layer
space
molecular layer
space

to give the type of picture which we have obtained.

Now at this stage we can already see an interesting comparison with the results from the X-ray work. The specimen for electron microscopy has been dried in a vacuum before being photographed, and we can assume that in this drying process most of the water is removed from the crystal. The picture we have obtained suggests that this water might well occupy the interlayer space in the crystal and that, on drying, this water is removed, thus giving the observed layered structure. Confirmation of this comes from unit cell measurements of both wet and air-dried crystals of this form of ribonuclease which have been made by Carlisle and Scouloudi and by Magdoff and Crick. The values they obtained are shown in Table I.

TABLE I

	CARLISLE AND SCOLOUDI		MAGDOFF AND CRICK	
	Air dry	*Moist*	*Air dry*	*Moist*
a	29·1	30·9	30·4	30·5
b	30·1	38·8	33·4	38·3
c	51·0	54·1	53·0	53·3
β	114°	106°	113°	106°
a sin β	28·0	29·3	26·4	29·3

The interesting point here is that although there are fairly wide differences in the individual values obtained in the two sets of measurements, the greatest difference between wet and air-dried crystals occurs in the *b* measurement in both cases. This agrees very well with the structure we are now suggesting, from the electron micrograph.

The X-ray investigation has shown that there are two molecules in the unit cell, and, since the observed spacing of 18 Å is half the *b* axial spacing observed by X-ray diffraction, this suggests that this spacing of 18 Å is in fact the intermolecular spacing and that the dark bands are in fact the layers of molecules in the crystal. Where we can in some regions see a further splitting of the 18-Å dark band into two 9-Å sub-bands, it is tempting to suggest that here we are beginning to see evidence of the two component polypeptide chains in the molecule, but this will require further confirmation from other micrographs taken on fresh crystals before we can be quite sure.

There still remain the interesting, but as yet unexplained, broad bands which are spaced about 140 Å apart and which, as you can see, vary in spacing and in direction over the crystal fragment. It was perhaps fortunate that at this time we were also looking at another and much simpler material, namely graphite, at high resolution in the microscope. The graphite structure is a simple one, consisting, as it does, of a layered structure. Figure 4 shows the arrangement of the carbon atoms in the graphite structure, and when we look down on this structure from above we see that it is composed of a carbon network in which we have rows of carbon atoms arranged along three directions, each inclined at 60 degrees to one another, as in Figure 5. When we look at small graphite crystals in the electron microscope, we cannot resolve these individual rows of carbon atoms, but we do however see rows of lines, and this pattern is derived from the underlying structure, as in Figure 6, plate 1, a photograph of graphite taken by Mr. E. Follett. This is known as a moiré pattern and it is quite simply derived from the superposition of layered structures.

The production of this moiré pattern can best be understood by considering as a model something very much simpler than either the ribonuclease structure or the graphite structure; the simplest structure of all: a series of fine lines ruled on a glass plate. If, as in Figure 7, we superimpose two such identical ruled sets of lines, inclined at a small angle to one another, then a third pattern emerges. This third pattern is known as the moiré pattern. The spacing of these moiré fringes, as they

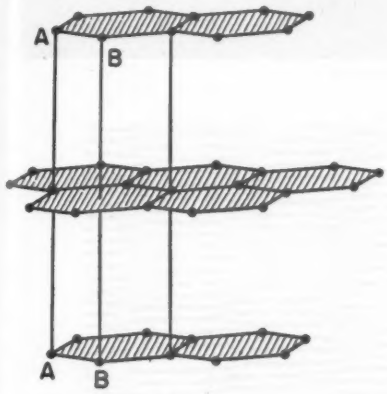

FIG. 4

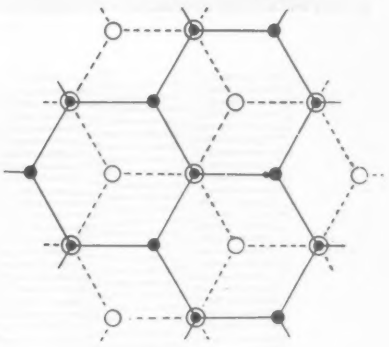

FIG. 5

FIGURES 4 & 5—Structure of graphite (J. M. Robertson, *Organic Crystals and Molecules*. Cornell U.P. 1953)

are called, varies with the angle between the two ruled patterns, and as we rotate the one set of lines with respect to the other, we can vary the spacing between the secondary bands. Now this is the very simplest kind of optical analogy to what is happening in the graphite and the ribonuclease crystals. The pattern seen in both these cases is much more complex than this simple one demonstrated in Figure 7. This complexity can be introduced by having a multiple number of plates with ruled lines all set at different angles, or by having sets of lines with differing spacings ruled on them. But just as in graphite they can be related back to the structure of the graphite lattice itself, so in ribonuclease can they similarly be related to the structure of the ribonuclease molecule.

As we have shown, however, moiré patterns, although related to the basic structure of the crystal, have spacings which are not simple multiples of the basic molecular dimensions, since the spacing observed depends on both the molecular

FIG. 6, PLATE 1

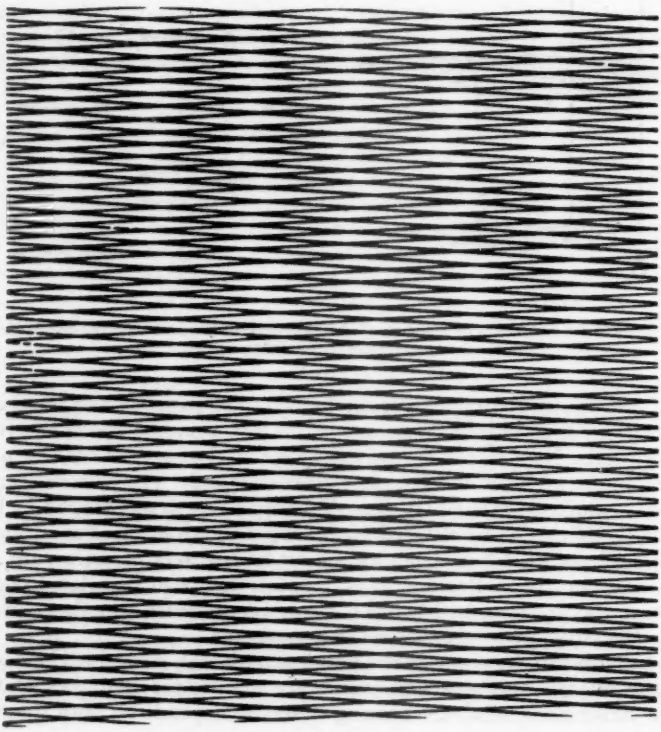

FIG. 7

spacing and also the angle between the two components. Since this angle of rotation can in most cases be infinitely variable, the moiré spacing is variable. However, the occurrence of moiré fringes indicates a regular, ordered structure within the lattice down to quite small dimensions. In ribonuclease this suggests a high degree of order within the molecule at quite small dimensions.

These results are now being correlated with the X-ray measurements, and it is hoped thereby to advance our state of knowledge of the structure of this particular protein ribonuclease.

THE ORIGIN OF VERTEBRATES—MARINE OR FRESHWATER?[1]

by

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IN this paper I am not going to add much in the way of new facts, but will rather attempt to interpret old ones. We all know that the vertebrates or backboned animals are found today in all the major environments, in the sea, in fresh waters, and on land. But did the earliest vertebrates, presumably fish-like forms of some kind, live in the sea or in brackish or fresh water? How can there ever be proof of their first environment? As I see it, there can never be any certitude or finality about it: any conclusion about the habitat of those first vertebrates which developed from soft-bodied invertebrates or from protochordates must remain one of probability only.

This conclusion will depend on a consideration of two main types of evidence, the more direct data from palaeontology (the study of the early fossil remains in the rocks), and the inferences which may be drawn from a study of the structure and physiology of present-day lower vertebrates and other chordate groups.

The first vertebrate remains recognised in rocks of Ordovician age in Colorado and Wyoming, estimated to be about 400 million years old, are pieces of bony skeleton and bony scales belonging to the group of fish-like creatures known as ostracoderms. Many more records of the jawless ostracoderms and of jawed acanthodians (which were true fishes) are known from the next period, the Silurian, from rocks in both Europe and America.

A question which must be settled to begin with is what was the salt content or salinity of the ancient Ordovician seas. At first we might think that all waters, including the seas, would be relatively fresh, since this was a long time ago, and the present-day seas have presumably slowly accumulated their salts chiefly from those brought in by rivers. But then we remember that even in the earlier Cambrian rocks there were many marine invertebrates, including jelly-fishes and echinoderms such as crinoids and sea-cucumbers which at the present day cannot survive dilute sea water. Also, I think it is reasonably well established on geochemical grounds that the sea at the beginning of the Ordovician period was almost as saline as it is now. Consequently, the invertebrates and chordates which lived then were under the same physico-chemical environmental stresses as affect these animals today.

The calculations of Professor Conway (1943) on the chemical evolution of the ocean take into account many facts: the composition of igneous and sedimentary rocks; the composition of river waters and the proportion of their ionic constituents which is derived from sea-spray and rain; the solubilities of the various salts. On one hypothesis the chloride (Cl) is assumed to have come from volcanoes, and to be increasing continuously. On this basis the total ionic concentration at the beginning of the Ordovician period is calculated to be 84 per cent of the present-day value for a salinity of 35 gm./kg. (19·8 gm. Cl/litre). On another hypothesis chloride is assumed to have condensed as hydrochloric acid with the water of the oceans from the original atmosphere. On this basis the ionic concentration is about 99 per cent of the present value. (These two calculations are based on constant oceanic volume.) A third hypothesis, that both the chloride and the water of the oceans have come

[1] Paper read to Section D (Zoology) and Section I (Physiology and Biochemistry) at Glasgow, on September 1, 1958.

from volcanic sources leads to a composition in the Cambrian and Ordovician periods closely resembling the present ocean.

The removal of calcium continuously as the carbonate and sulphate salts, removal of magnesium as carbonate, and potassium mainly as the mineral illite (Conway, 1943; Hutchinson, 1944) mean that in all probability the relative ionic ratios in the early Palaeozoic seas were very similar to what they are now.

Returning now to the evidence from palaeontology, three recent workers have investigated this and have come to the same conclusion independently. Denison (1956), myself (1957) and Errol White (1958) all agree that the habitat of the earliest vertebrates was probably the coastal waters of the sea. Chief support for this is the fact that the vertebrate remains found in sedimentary rocks of Ordovician and Silurian age are mostly associated with such marine invertebrates as brachiopods, trilobites, cephalopods, crinoids and corals. Also there is no real evidence of Continental deposits in the Ordovician and Silurian periods and no evidence whatever that any of the invertebrates associated with the armour and scales of the ostracoderms and acanthodians were freshwater animals. Yet all our American and English textbooks today state that the vertebrates were probably freshwater animals. They accept the views of Homer Smith (1932) and Marshall (1934) that the peculiar vertebrate kidney, the glomerular kidney, probably arose in freshwater vertebrates or pro-vertebrates. So strongly does Professor Romer believe this that in a recent paper (1955) he has declined to accept the conclusions of palaeontologists who have investigated the vertebrate remains, and has substituted a view that these remains in marine Ordovician and Silurian deposits were carried into them after the death of the animal in rivers.

This is, of course, a possible interpretation, and is one of the reasons why the question is difficult to decide on palaeontological data alone. However, this view has no direct supporting evidence. I think that workers like Romer and Homer Smith are so impressed by the freshwater habitat of some of the Devonian ostracoderms and fishes, when lungs were being evolved in the bony fishes, for example, that they project back, in a quite unwarrantable way, the conditions during part of the Devonian period to the earlier Ordovician and Silurian periods.

Let us now see how far other types of evidence bear on this question of the early habitat of these ostracoderms, acanthodians, and their ancestors. Here we are considering inferences drawn from studies on present-day chordates, including the lower vertebrates.

First of all, the habitat of the animals grouped with the vertebrates in the Chordata. The Hemichordata (*Balanoglossus* and its allies), the Urochordata (or tunicates) and the Cephalochordata are all marine. Only the Cephalochordata, however, *Amphioxus* are related genera, are close to the vertebrate line, and perhaps they show in their shape, arrangement of muscles, blood-vessels and nervous system a stage of pre-vertebrate organisation through which the cyclostomes and fishes may have passed. My argument here would be that if the vertebrates arose from one of what we may call the invertebrate chordate groups, this is most likely to have occurred in the sea, since none of the lower chordate groups penetrate fresh water.

The typical spindle shape of a fish like the salmon or haddock has been taken by some people to have evolved in a fluviatile environment. But few of the first vertebrates in the palaeontological record had a stream-lined form. The idea that this form necessarily arose in rivers is one we cannot accept. *Amphioxus* has such a form and it is marine. Besides, we have the adoption of a fish-like form in the sea, in some of the higher vertebrates, in the whales and dolphins for example, and in the extinct marine reptiles, the ichthyosaurs. I think that a fish-like form and fish-like swimming movements were essential for the initial invasion of streams by marine vertebrates.

The next point I wish to consider is the composition of the blood. There is a general resemblance in ionic composition between the blood-plasma of chordates and sea water. This suggests that an internal fluid of the general composition of sea water is necessary for the proper functioning of chordate tissues. To use Macallum's (1910) phrase, the blood-plasma is, in a certain sense, 'the heirloom of life in the primeval ocean'. It reflects in its composition the past adaptation of tissues of chordate and prechordate ancestors to an internal medium of relatively the same composition as sea water.

All the marine invertebrates, except certain crabs and most prawns, have a total concentration of salts equal to that of sea water. So have the tunicates. The salt

TABLE I

Relative ionic composition of the plasma or serum of aquatic chordates in relation to sea water (Robertson, 1957)

	Habitat	Total ions relative to sea water	Weight units					
			Na	K	Ca	Mg	Cl	SO_4
Sea water	—	100	55·5	2·01	2·12	6·69	100	14·0
Urochordata								
Phallusia (tunicate)	marine	100	53·3	1·95	1·91	6·38	100	7·1
Cyclostomata								
Myxine (hagfish)	marine	98	62·9	1·84	1·23	2·31	100	3·1
Lampetra (lamprey)	freshwater	21	80·9	3·69	2·32	1·51	100	7·7
Osteichthyes								
Muraena (Roman eel)	marine	32	72·9	1·14	2·32	0·88	100	8·2
Coregonus (powan)	freshwater	25	75·7	3·48	2·50	0·96	100	5·1
Chondrichthyes								
Squalus (dogfish)	marine	53[1]	60·3	2·78	1·64	1·49	100	—

[1] Total particles, including non-electrolytes urea and trimethylamine oxide, come to *ca.* 100–102.

concentration of the plasma of all the aquatic vertebrates except the myxinoid cyclostomes (the so-called hagfishes or borers) and the elasmobranchs, is about one-quarter to one-third that of sea water. This osmotic regulation implies that the ancestors of these animals were freshwater in habitat at some stage; those which returned to the sea, like the ancestors of the teleost fishes, still kept the lower salt content of the internal medium acquired during their sojourn in fresh water.

In the marine elasmobranchs (the sharks and rays) the salt content of the plasma is about one-half that of sea water, but the total particle concentration is made up to that in sea water by the presence of urea and trimethylamine oxide. The elasmobranchs are believed to have been ocean dwellers since the beginning of their known history (Romer, 1945).

Unlike the teleost fishes and the elasmobranchs, the myxinoids maintain osmotic

equilibrium with sea water by means of a high concentration of salts in their plasma. In this respect they are like the lower marine chordates, such as the tunicates, and the marine invertebrates. I think that this feature in the myxinoid order of the most primitive living vertebrate group, the cyclostomes, may well be a primary one, being derived directly from marine chordate ancestors, and not a secondary derivation from unknown freshwater ancestors.

It is now accepted that the present-day cyclostomes are related to the extinct ostracoderms. The distinguished Swedish palaeontologist Stensiö (1958) firmly believes in a relatively close relationship between the Myxinoidea and one of the ostracoderm groups, the heterostracans, of which the oldest fragments are in the marine Harding Sandstone of Colorado, constituting the oldest authenticated vertebrate remains, of Middle Ordovician age.

I pass on now to the last point—the glomerular kidney of vertebrates. The kidney consists of a large number of nephrons or renal tubules, each consisting of a tuft of blood capillaries closely applied to the blind end of the tubules proper. The hydrostatic pressure in the blood capillaries causes a protein-free fluid to pass across into the tubule where it may be altered in composition by reabsorption and secretion processes. In man there is nearly a million of these nephrons in one kidney, and in a fish like the cod, for example, about 24,000 in a specimen of 1 kg. weight.

Because of the usefulness of a filtration mechanism for getting rid of water, Marshall and Homer Smith have suggested that the glomerular kidney arose in freshwater protovertebrates as a means of excreting the large quantities of water entering the body in accordance with the osmotic gradient. They also suggest that in the teleosts which have gone back to the sea, the glomerulus is being reduced and will eventually disappear, as has happened in a few of them, at least in the adult state. This would be in accordance with the now reversed osmotic gradient between the blood and sea water.

Some of the common text-books incorrectly state that glomeruli are few or often completely absent in the kidneys of marine teleosts. Actually, aglomerular and almost aglomerular families comprise only about a third (17 out of 46) of those investigated, and common successful families like the gadoids (haddock, etc.), herrings, plaice and mackerel all have numerous glomeruli. (Edwards, 1928; Nash, 1931; Robertson, 1954.)

Although the filtration surface of the kidney, or a figure based on the volume and number of glomeruli per unit area of body surface, is reduced in marine compared with freshwater teleosts, it is not reduced in the marine elasmobranchs and in the myxinoids. These two groups have filtration surfaces as large or almost as large as those of freshwater teleosts, and their blood is isosmotic with sea water—they have no osmotic gradient, at most 2 per cent.

From these facts I conclude that the glomerular kidney is not necessarily a freshwater adaptation. It is very useful in the freshwater habitat, but it was probably present in marine protovertebrates. On this hypothesis marine immigrants to fresh waters found it a useful, almost an essential, type of organ, one pre-adapted, as it were, for use in fresh water. Supporting this interpretation is the fact that analogous organs, based on the same principles of selective absorption and secretion are found in marine invertebrates. These organs in animals of marine origin and habitat, such as marine crabs and cephalopods, are able to secrete relatively large amounts of water, although the blood and sea water are in equilibrium osmotically. The point I wish to make is that these organs (antennal glands in crabs, renal tubules in cephalopods) have evolved in marine invertebrates with blood isosmotic with sea water, and a similar possibility is open for the evolution of the glomerular kidney in marine protovertebrates.

We now see that the final conclusion must be based on probabilities, since two

directly opposing viewpoints may be maintained. I think I have made clear my reasons for favouring an original marine habitat for the vertebrates. In conclusion, the palaeontological evidence that the Ordovician ostracoderms and the Silurian ostracoderms and acanthodians were probably marine is not opposed by the inferences which may be drawn from structural and physiological features of present-day vertebrates. Among these are the following: The lower chordate groups are marine, and so probably were the first vertebrates. The fish-like form of the early vertebrates may well have arisen in the sea. The equivalence in salt concentration of myxinoid plasma and sea water may be a primary feature in this most primitive of vertebrate groups, the myxinoid cyclostomes, a feature derived from marine chordate ancestors. Lastly, the glomerular kidney is not necessarily a freshwater adaptation. It may have existed in marine protovertebrates, being a useful, if not almost essential, organ in an animal entering brackish and fresh waters from the sea.

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THE WORK OF THE FRESHWATER BIOLOGICAL ASSOCIATION

SECTIONS D (Zoology) and K (Botany) met together during the morning of Tuesday, September 2, 1958, at the Glasgow Meeting of the British Association to consider a number of papers outlining the work of the Freshwater Biological Association. Professor W. H. Pearsall, F.R.S., Emeritus Professor of Botany, University College, London, reviewed the history of the Association in a paper entitled 'The Freshwater Biological Association and its laboratory'. He was followed by Dr. C. H. Mortimer, F.R.S., Director of the Marine Biological Station at Millport, who presented a paper on 'The physical and chemical work of the Freshwater Biological Association, 1935–1957'. Dr. J. W. G. Lund of the Marine Station at Windermere then delivered a paper which dealt with 'Investigations of the algae of Windermere'. The final invited contribution to the session came from Mr. E. D. Le Cren of the Marine Laboratory at Windermere who described 'Experiments with fish populations in Windermere'.

Shortened versions of each of these papers are printed below.

THE FRESHWATER BIOLOGICAL ASSOCIATION AND ITS LABORATORY

by Prof. W. H. Pearsall, F.R.S.

The history of the Freshwater Biological Association is very much bound up with the activity of the British Association, for the Freshwater Biological Association owes its existence to a discussion on freshwater biology, which took place, incidentally, in this room, at the corresponding meeting of the British Association thirty years ago. Moreover, the Freshwater Biological Association, since that time, has been indebted to the British Association for the annual grants from the Caird Fund with which to maintain a working table for visiting biologists. In many respects then, what I have to say about the Freshwater Biological Association is in the nature of a progress report to a parent body.

It may be prefaced, perhaps, by an attempt to summarise the reasons which led to the foundation of the Freshwater Biological Association. Great Britain, containing more than 500 square miles of freshwater surface, had always been in an exceptionally favoured position for scientific work in this field and, in some respects, had led the way in handling freshwater problems. The work of the Rivers Pollution Commission in the 1860's had provided a body of data on water types and quality, which was not only of scientific interest but which had enabled this country to develop public water supply to a level greatly in advance of most others. Here, in Scotland, Sir John Murray had inspired a bathymetrical survey of the Scottish lochs, which was of the greatest value and which had been followed by E. M. Wedderburn's work on lake temperatures and seiches. Unfortunately, these great efforts were accompanied only by somewhat modest biological surveys, dealing with plants and plankton crustacea. Later, Dr. H. R. Mill had made a similar bathymetrical survey of the English Lakes and W. and G. S. West had been engaged on a systematic survey of freshwater algae over a long period of years.

Nevertheless Britain lagged far behind other European countries in the provision of laboratories for research on fresh water. Such work as was being done, centred largely on a few Universities and was of necessity taxonomic and survey work, of a sort which did not require laboratory services. The First World War,

however, produced a revolution in biological thought. The classical training in comparative anatomy and morphology, which was a legacy of T. H. Huxley's teaching, had to face a new generation of biologists, many of whom had been shaken out of their academic mould by the rough impact of war service. The new elements were attracted much more keenly by the experimental, ecological and genetical aspects of the subject, and it was in this changed climate of biological thought that the movement for a freshwater biological laboratory matured.

My own first contact with this possibility was in 1924, when I met at dinner J. T. Saunders, then in the Zoology Department at Cambridge, who was completing a survey of the factors controlling the pH of natural waters and was also interested in developing new methods of measuring temperature in lakes. We agreed, then, to work for a field laboratory. Others, however, had reached the same conclusion. Mr. F. T. K. Pentelow had been working, in 1925, at Alresford for the Ministry of Agriculture and Fisheries, who a year later started a biological survey of the River Lark, then being polluted by the effluents from sugar beet factories. In 1927, Professor F. E. Fritsch, in his Presidential Address to Section K (botany) of the British Association at Leeds, dwelt on the lack of laboratory facilities for research in freshwater biology in this country, and in the following year (1928), Sections D (Zoology) and K (Botany) of the British Association had a joint discussion, with Professor Fritsch in the chair, on the biological investigation of fresh water. This discussion led to the appointment of a committee to report on the best means of promoting the establishment of a laboratory. The report was in favour of the institution of an association, modelled very much on the lines of the Marine Biological Association, and the formation of such an association, the Freshwater Biological Association, was agreed on at a general meeting held in the Linnean Society's rooms on June 11th, 1929.

Two matters were the basis of much discussion, the possible site of the new Laboratory and the policy to be followed in research. As to the former, there was much support for two possibilities, a site on the Norfolk Broads and one on a southern river, perhaps, preferentially on an unpolluted chalk stream, while a third possibility, some pools at Sutton Coldfield not too far from the river Severn, was also explored. It was felt however, that moving water introduced too many difficulties at the preliminary stages, while many of the Southern waters reproduced conditions already under investigation on the continent. The final choice thus fell on Windermere, as offering access to lakes and tarns of all sizes, which included some of the distinctive Caledonian type, as well as a very great variety of other aquatic habitats within reasonable reach.

The other question of policy decided, was that the Association would pursue fundamental scientific questions in its research, and would not undertake detailed enquiry into *ad hoc* fishery or water-supply problems. There were, indeed, those who wished even to rule out applications for financial help to angling bodies or water-undertakings, but wiser counsels prevailed, and the Association soon came to recognise that the far-sighted among those interested in fishery problems and water-supply were among its most valuable supporters.

Meanwhile, a worse time for starting a new venture could not have been chosen, The country was in the throes of a great industrial depression and in the middle of 1931, Britain went off the gold standard. There was no possibility of building a laboratory, but the Association was fortunate in obtaining three suitable rooms and a boat-house at Wray Castle, a choice example of Victorian Gothic by the side of Windermere, and there the laboratory was opened in September 1931. In order to do this, apart from members subscriptions, the Association had grants from the Royal Society and the Fishmongers Company, while the Development Commission, by contributing £450 in March 1931, had approximately trebled the annual income.

Although the omens were unpropitious, subsequent events were to justify our optimism and except for the years of the Second World War, to show steady and continuous progress in the work of the laboratory. The whole of Wray Castle was taken over in 1933, and in 1937, after inspection of the laboratories, the Development Commission raised the annual grant to £2200, in order to allow of the appointment of a full-time Director, Dr. E. B. Worthington. On his departure to Africa, in 1946, Mr. H. C. Gilson, the present Director took over, and as Wray Castle was becoming increasingly congested, he was immediately involved in the acquisition of another building, the Ferry Hotel (1947) and its conversion into a laboratory. It was formally opened by the Chairman of the Development Commission, the Countess of Albemarle, in 1950, and has proved a good acquisition. The International Limnological Congress was held in Britain, in 1952, partly at the Ferry House Laboratory. The Congress awarded the Laboratory the Einanr Naumann Medal, in recognition of its many and valuable contributions to limnology.

It may be appropriate to indicate briefly, what contributions the laboratory and its staff have been made to limnology and to biology in general. The Association was extremely fortunate in beginning with two extremely able young men like P. Ullyott and R. S. A. Beauchamp, as its first naturalists. Ullyott was soon to return to a Cambridge fellowship—but Beauchamp (now Director of the Fishery Research Laboratory on Lake Victoria) stayed until 1937. Their work on flatworms was almost a model of what ecological investigations could be, defining the problem in the field and then taking it into the laboratory to solve it under controlled conditions.

Experimental ecology, a combination of field analysis, laboratory control and experiment, and then field experiment and verification, has since then remained a constant and valuable feature of the work of the laboratory, both with plants and animals. The impact of this has, I think, been greater in zoology than in botany for, during the early days of the laboratory, Charles Elton's population studies at Oxford, represented almost the only animal ecology in Britain. Habitat ecology on the animal side was almost non-existent and the laboratory has thus led the way in establishing this aspect of biology in Britain. It is appropriate, therefore, that the present director should now be Editor of the Journal of Animal Ecology.

Equally valuable, however, to limnology and freshwater biology have been two other aspects of the work, the wide surveys of habitat conditions, particularly of water chemistry, made possible by Mr. F. J. H. Mackereth's analytical enterprise, and the long term observations on animal populations, of corixids, of stream insects and of shore faunas, carried on by Dr. T. T. Macan and his colleagues, and based on a profound and laboriously acquired knowledge of the immature stages of the characteristic invertebrates.

Three other aspects of the scientific work are to be outlined by others, so that I need say nothing about them now, but on looking back over the work of the laboratory, I think that three things in particular, made for success. The first of these was enthusiasm, the basis for the necessary hard work. Many would agree that the second has been the success of the Easter classes in inspiring others, and in training recruits. The third, without question, has been the ability of the staff to seize upon the significant. I could elaborate on this theme, at length, if space permitted and were I not sure that you will find many illustrative examples, in following the ensuing papers.

THE PHYSICAL AND CHEMICAL WORK OF THE FRESHWATER BIOLOGICAL ASSOCIATION, 1935–57

By Dr. C. H. Mortimer, F.R.S.

Regular physical and chemical observations on Windermere and nearby lakes began in 1935 with the appointment at Wray Castle of a research student (zoologist) required to spend part of his time on the chemical analysis of lake waters. This student—the writer—entered upon this work with a schoolboy's knowledge of chemistry and only moderate enthusiasm. But enthusiasm grew under the influence of stimuli from varied sources; and the study of the properties of the lake environment became a full-time occupation which lasted (with a break during the war) until 1956. Among these stimuli, which also provided a starting point for the work, may be numbered: Pearsall's demonstration of causal links between catchment soil types, water chemistry, and the algal floras of the English Lakes; continental and American studies of seasonal events, particularly stratification, in lakes; and the attention which had been paid, largely by marine workers, to those essential plant nutrients (notably nitrogen and phosphorus) which are normally present in great dilution and which may therefore be expected to exert a controlling influence on the rate of production of life in natural waters.

A preliminary, naive attempt (1) to estimate quantities confirmed, as Pearsall (2) had shown, that spring sunshine brings on an outburst of diatom growth in surface waters and that this is accompanied by a fall in concentration of dissolved silicate, nitrate and perhaps phosphate, although the extreme dilution of phosphate (*ca.* 1 mg/m^3) made it impossible to be certain about this. It was found, in other words, that the production cycle was similar to that observed in many other bodies of natural water, marine and non-marine, and that the short supply of some essential nutrient or nutrients apparently exercised control over the quantities of living material produced. Looking back over twenty years' work, it is apparent that some components of this mechanism have been the subject of more intensive research than others, and that this research has been directed along three main lines. Limitations of space allow only one example from each line to be given here.

(i) Regular, Quantitative Observations in Selected Lakes

Pearsall's studies of the seasonal cycles in phytoplankton crops, underwater illumination (3), and water chemistry (2) in the English Lakes formed the background for the early work at Wray Castle. To my regular observations, which covered the years 1936 to 1940, should be added those of C. B. Taylor (on the staff 1938–42, 1946–8) and his assistants in connection with bacteriological studies (4). But the first close study of seasonal correlations between algal members and concentrations of certain nutrient salts began with the arrival at Wray Castle of J. W. G. Lund in 1944. At first Lund carried out all the work, including the chemical analyses, alone. He later had the assistance of others, including the chemists F. J. H. Mackereth (appointed 1946) and J. Heron (1951), and this close watch on nature has continued as a team effort for more than ten years. Some aspects of this work will be discussed by Lund in the next section. Here, with the help of figure 1 and with particular reference to physics and chemistry, I will briefly indicate some factors which have been found to control the population density of the diatom *Asterionella*.

(*a*) *Physical control.* The basins (a), (b), and (c) are placed in order of depth (maximum: 15, 33, and 65 m., respectively). In each lake and year the *Asterionella* population increased with increasing sunshine in early spring. At this season the basins were not stratified, the cells were carried by turbulent currents to all depths, and the average time which each cell spent in the illuminated surface layers, where

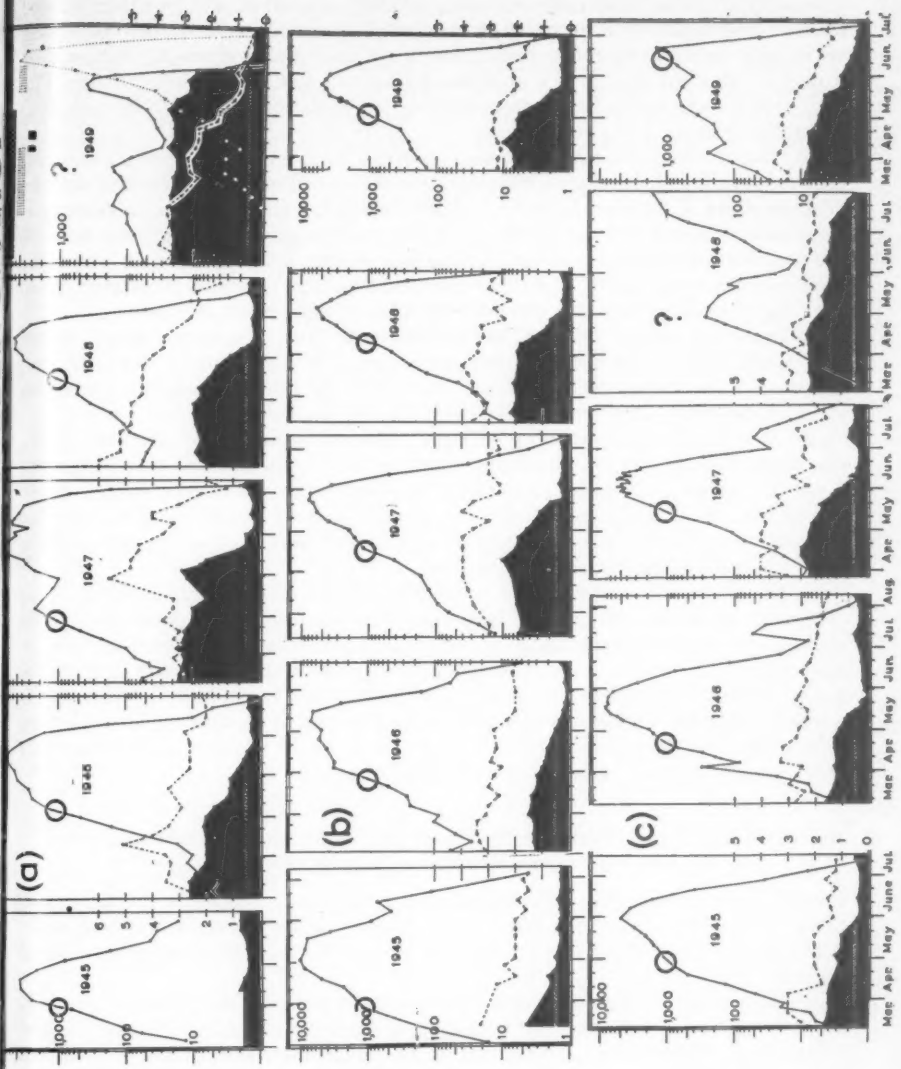

Fig. 1, after Lund (5). Spring growth of *Asterionella formosa* in the upper 5 metres of three lakes: (a) Esthwaite Water, (b) Windermere, southern basin, and (c) Windermere, northern basin. Cell numbers are given (unbroken lines) on a logarithmic scale, 1 to 10,000 per ml. Concentrations of dissolved silicate (as SiO_2, black area) and nitrate (as N, broken line) are shown on a linear scale, 0 to 5 mg./l. The horizontal shaded lines within the black areas, indicate the critical concentration of 0·5 mg./l. SiO_2. In the top right-hand diagram (Esthwaite Water, 1949) the hatched area indicates abundance of the blue-green alga *Oscillatoria*, the dotted areas indicate severe parasitization of *Asterionella* by fungi, the black rectangles indicate floods, and the dotted line shows cell numbers of other diatoms; *Fragilaria* and *Tabellaria*. For further details see Lund (5). The large circles show the dates on which the *Asterionella* population had reached 1000 cells/ml., and the question marks indicate exceptions when the 'normal' spring increase did not take place.

it could grow, was presumably inversely related to basin depth. Bearing in mind that the daily totals of illumination entering the lake surface were steadily increasing at this season, it was therefore reasonable to conclude that a given population level would be attained first in the shallowest lake and last in the deepest one. The dating of the circles in Fig. 1 (representing a population level of 1000 cells/ml.) generally confirmed this conclusion.

(*b*) *Control through a major inorganic nutrient.* In most of the 'spring increases' illustrated in Fig. 1 the silicate concentration fell as cell numbers increased and the peak numbers of between 5,000 and 10,000 cells/ml. coincided with a silicate concentration of about 0·5 mg./l. SiO_2. At this concentration of silicate and cells (as the analysis quoted later shows), not enough silicate was available for one more division of the population, and there followed a sudden mortality. However, there were exceptional years which provide examples of other types of control.

(*c*) *Biological control.* During 1949 in Esthwaite Water, heavy parasitism of the *Asterionella* by Chytrid fungii (dotted rectangle in the Fig.) kept the numbers low; and it was not until two other diatoms (*Fragilaria* and *Tabellaria*, dotted line in the Fig.) appeared on the scene, that the silicate fell to low levels.

(*d*) *Trace nutrient control.* The spring increase in Windermere, northern basin, was halted during very dry weather in 1948, apparently for the lack of an unknown, possibly organic, trace nutrient normally supplied by inflow waters. After a flood, diatom growth restarted and reached the normal limit set by silicate concentration. It appears that diatoms cannot live by mineral 'bread' alone but also require organic trace materials of the nature of 'vitamins'.

A fall in nitrate concentration, occurring during the spring, appears to be unrelated to the diatom growth. It is paralleled by similar changes in the inflow waters, and probably depends on processes in the catchment soils and not in the lakes. Indeed, one of the important general results of the Association's chemical work, which includes recent studies by Mackereth (6), Heron, and Gorham on the origins of dissolved substances in rain, upland tarns and streams which I have no time to mention here, has been to direct attention to the importance of catchment processes in the understanding of lake chemistry.

(ii) Laboratory Studies: Analyses of Algae and Determinations of Their Essential Requirements

It may appear strange that 'phosphate control' was not included in the above list, although the concentration in Windermere is near the limit of detectability at about 0·001 mg.P/l., and phosphate control has been postulated for many other natural waters.

Mackereth has determined that a million cells of *Asterionella formosa*, representing 0·3 mg. dry weight, contain: 0·14 mg. SiO_2, 0·06 mg. C, 0·007 mg. N, and 0·00006 mg. P (*minimum*). It can, therefore, be calculated that there is sufficient phosphate in Windermere to support at least double the population found at the spring maximum, which suggests that, there at least, the low phosphate concentration does not limit growth. Mackereth (7) has shown that the situation is further complicated by this diatom's habit of taking up phosphorus in excess of its minimum requirements—a 'luxury consumption'. Other algae probably do the same. During the early growth phases in the culture experiment illustrated in Fig. 2, the phosphorus content per cell was high, but fell to the above-quoted minimum value when the population had reached a maximum number of 16×10^6 cells/l. and growth had stopped. Addition of a further 0·001 mg. P/l. was immediately taken up by the cells (thereby doubling the P concentration per cell); growth re-started and continued until the population had reached 32×10^6 cells per litre and the cell concentration had again fallen to the same minimum value.

(iii) Mechanisms Regulating Supply and Transport of Nutrient Materials in Lakes

It had been observed by Pearsall, and in more detail by Miss P. M. Jenkin (8) working at Wray Castle in 1934, that Windermere—in common with other temperate lakes—became stratified in spring into two main layers separated by a thermocline. During stratification, the uptake of nutrients by plants was largely confined

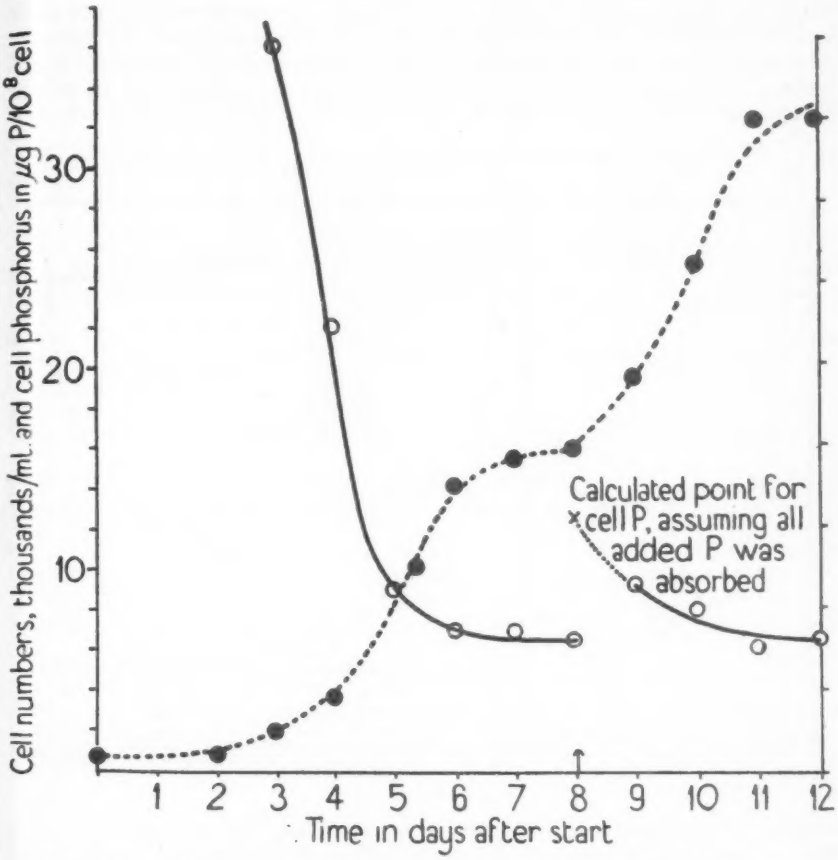
Cell numbers, thousands/ml. and cell phosphorus in µg P/10⁸ cell
30
20
10
Calculated point for
× cell P, assuming all
added P was
absorbed
1 2 3 4 5 6 7 8 9 10 11 12
Time in days after start

Fig. 2, from Mackereth (7). Growth (cell numbers shown by broken line) and phosphorus content (unbroken line) of *Asterionella formosa* in a culture of lake water in which the initial phosphate concentration was 0·001 mg./l. P and in which ample silicate had been provided. At day 8 a further 0·001 mg./l. P was added as phosphate.

to the upper layer. My own work has been mainly concerned with the mechanics and the effects of this stratification. The upper layer is the scene of synthesis of organic matter up to the limits of the nutrient supply; it is well-aerated and warm. The lower layer is cold, is cut off from exchange with the atmosphere and is the scene of the decomposition of organic matter trapped there by stratification or falling from above, accompanied by depletion of dissolved oxygen. This depletion may be rapid if the amount of organic matter is large or if the depth and volume

of the lower layer is small, in which case the trapped store of dissolved oxygen may disappear before the summer is over and before autumn cooling and autumn winds destroy the stratification and allow oxygen to be carried right to the bottom.

The effect of these oxygen changes on physico-chemical events in the lower layer and particularly in the top few millimetres of the sediments occupied my main attention from 1939 to 1941 (9). These events are of some importance in regulating the liberation of the products of decomposition, i.e. potential plants nutrients, into the water. For instance, phosphate is strongly adsorbed on ferric hydroxide which is precipitated at the sediment surface as long as the water in contact with it contains oxygen. If the oxygen disappears, the ferric iron is reduced to the soluble ferrous form and phosphate is liberated. A description of the striking changes associated with the seasonal oscillation between oxidation and reduction

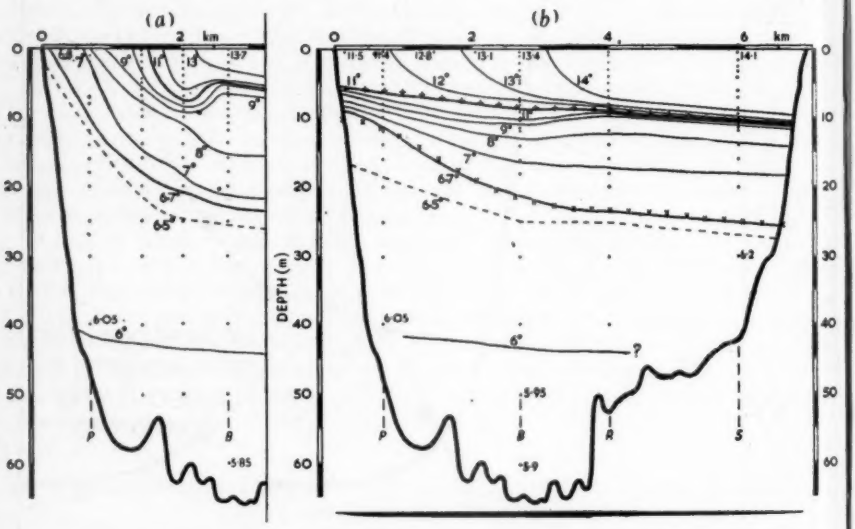

Fig. 3, from Mortimer (12). Distribution of temperature along the axis of Windermere, northern basin, on 9 June 1947, (a) at the height of a storm (Beaufort force 8, blowing from left to right in the diagram), and (b) 5 hours later when the storm was moderating. The black silhouette at the base of (b) represents the section in its true proportions.

in the lower layers of some lakes would take too long, but in conclusion I will mention some results of a search, promoted by these chemical studies and continued after the war, for mechanisms responsible for the transport of materials by turbulence in the lower layer and by occasional exchange across the thermocline into the upper layer.

It would be difficult to produce an adequate picture of motion in a lake by current measurements alone, and it is therefore convenient that the rates of gain or loss of heat from stratified lakes, except close to the surface, are sufficiently slow to allow short-term changes in temperature distribution to be used to infer motion. Clues provided much earlier by Murray (10) and Wedderburn (11) from Scottish lochs were followed up by detailed investigations in Windermere and other lakes using thermistor thermometers as probes for synoptic surveys of the whole basin or moored to record temperature at selected depths. Examples of the information provided by these methods are given, respectively, in Figs. 3 and 4.

The isotherm distribution in Fig. 3 was produced by a strong and steady wind blowing along the axis of the basin. The warmest layer (14° C. and above) was dragged to the downwind end, and the lower, colder and denser layers executed a compensatory motion upwind, which brought water below 7° to the surface at the height of the storm. This is a typical example of the disturbance of hydrostatic equilibrium produced by wind action. When the wind is followed by a calmer period, the disturbed layers move back to positions of new equilibrium, but oscillate to and fro before that equilibrium is reached. These oscillations are accompanied by an internal wave motion of the thermocline, which can be conveniently recorded by thermometers moored at appropriate depths near the end of the basin. Fig. 4 illustrates this for basins greatly differing in size and demonstrates the important conclusion that each basin tends to oscillate at its own 'resonant'

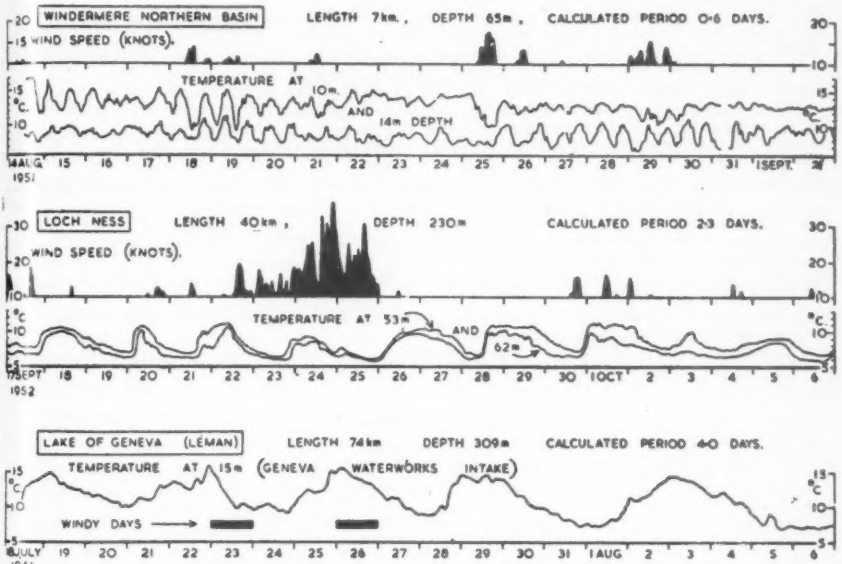

Fig. 4, from Mortimer (13). Twenty days of temperature records from depths near the thermocline, compared with records of wind (hourly mean velocities), in three lakes of different size.

frequency, determined by the dimensions and densities of the layers. The Windermere records, in particular, illustrate how the resonant oscillations are 'excited' during windy spells and how they decrease in amplitude during intervening calmer intervals. Subject to the complexity of wind and weather, these motions not only transport materials from one part of the basin to another but also, by virtue of the turbulence associated with the flow, cause a good deal of mixing and carry nutrients and other substances from one level to another.

To conclude this very brief and incomplete account, mention should be made of the lake sediment studies which started in 1938 with the exploration (14) of what appeared to be layers of glacial and post-glacial sediments disclosed by echo-sounding records (15). These studies, which still continue, have shed light on post-glacial climatic and vegetational changes (16) and, with the recent invention of improved coring equipment (17), will increasingly illuminate various aspects of lake history and present-day processes.

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Acknowledgments

Fig. 1 by courtesy of *The Journal of Ecology*.
Fig. 2 by courtesy of *The Journal of Experimental Botany*.
Fig. 3 by courtesy of The Royal Society.
Fig. 4 by courtesy of *Weather*.

INVESTIGATIONS ON THE ALGAE OF LAKE WINDERMERE

By J. W. G. Lund

It was natural that the algae should play an important part in the activities of the Association for in most lakes, and in many other waters, they form the base of the pyramid of life. Moreover the larger the lake the greater the importance of the planktonic algae, that is of those forms which carry out all or most of their growth in the open water. If the lake is large and deep enough its biological economy is mainly based on these algae. Thus a large lake may well be called an inland sea, and much of our work is basically the same as that of marine institutions. With this close link with marine biology it is not surprising that we have always had close relations with the Marine Biological Association at Plymouth. However the marine biologist is for the most part concerned with one vast world while for us there are innumerable small ones so our outlooks vary. In most of our worlds, planktonic algae are much less predominant than in the sea, and in many streams and rivers the flow is so rapid that there is insufficient time for any planktonic algae to grow. We, therefore, are often concerned with waters in which benthic organisms predominate.

The Association was fortunate in having two algologists as founder members whose interests covered every possible aspect of its work. Prof. Fritsch in 1928 had an international reputation and was to become the premier algologist in the world. Prof. Pearsall was going through the main algological phase of his many-sided career while his father too was keenly interested in these plants. It was in this period that Prof. Pearsall, in a number of papers about the plankton of the English Lake District, threw out a series of theories and hypotheses which are influencing our activities today.

Unfortunately the infant Association could not afford a botanist. In a pamphlet outlining its aims, the Council produced plans for a fine laboratory. However, even in 1950, two algologists, one mycologist and an assistant were working in the front half of a room in a piece of Victoriana called Wray Castle. The rear half of this room, small though it was, had to serve as the chemical laboratory.

Many of you are familiar with algae and will I know excuse me if I digress for a moment to touch on their nature for the benefit of those who are not. The algae embrace organisms which are obviously plants, such as the seaweeds and the green scums often seen in freshwater. Others are fairly obviously plants but may well appear to be somewhat curious. Many are neither clearly plants nor animals and some are animals. In the great group called the Chrysophyceae or golden-brown algae are organisms which can carry on photosynthesis, grow in the dark with suitable dissolved organic compounds, or take in and digest small objects such as bacteria, smaller algae or animals, oil droplets, starch grains etc. Indeed all three methods of nutrition may be carried out by one organism. Thus in the algae as a whole we pass by every conceivable gradation from the purest of plants, green as grass, dependent on inorganic substances in solution and sunlight, to those which are not so dependent but 'eat' other organisms and are equally clearly animals. Yet other algae are also included among the bacteria, such as several colourless Blue-green algae. Such an astounding diversity in biochemistry is paralleled by an equally astonishing diversity in size and form; from the famous alga of biochemistry *Chlorella*, a sphere which may be about 1/20000 in. in diameter, to the giant seaweed *Macrocystis* which is reported to reach 300 ft. in length. In the algae we can see before our eyes steps in evolution which might well have fascinated Wallace and Darwin.

All kinds are of interest to our Association, but for brevity I must confine myself to those more obviously plants and to plankton. Our main activities have, in the words of the booklet issued to define the aims of the embryo association, been concerned with the relations of plants to their surroundings and to one another, that is in the realm of ecology.

The existence and success of such photosynthetic organisms depend on numerous environmental factors; physical ones such as light and temperature and, as we have heard from Dr. Mortimer, water movements; chemical ones such as the supply of inorganic and organic nutrients; biological ones such as grazing by animals, parasitism by fungi and the production of substances by one alga or set of algae which inhibit or stimulate the growth of others.

However, before one can study the ecology of an organism one must be able to recognise it and, if possible, name it. Therefore taxonomic and floristic studies have played an important part in our work and we were fortunate to have the encyclopaedic knowledge and great experience of Prof. Fritsch at our disposal, while his unique collection of figures of freshwater algae is now in our laboratory and is being greatly enlarged. Most freshwater algae can be identified by examining natural populations brought into the laboratory. Some, however, like most bacteria, must first be cultivated in artificial media. The Council showed considerable foresight in appointing as the first algologist Dr. Rosenberg who had been trained in the new culture methods being developed on the Continent. It was soon clear that

the study of cultivated populations might well shed light on some of those ecological problems which are so difficult to solve by observations on natural populations, for the environmental factors could be studied separately or in selected groups under known controlled conditions. Some time after Dr. Rosenberg had to leave the Windermere laboratory, another foreign botanist, Dr. S. P. Chu, developed this ecological side. He improved the methods of growing plankton algae and devised a series of solutions more akin to natural waters. One of these in particular, with the addition of extracts of soil or metal-binding organic compounds, has proved to be the best general medium for freshwater algae yet devised. It or variants thereof are in use all over the world both for ecological and physiological work. I shall return to the nutrition of algae in my concluding remarks.

Today a vast number of estimations are being made to determine the amount of primary, that is plant, production in the sea and in freshwater in almost every part of the world. Photosynthesis is the basis of this and it is photosynthesis which is measured. Thus we are concerned with the amount of carbon incorporated into plants, mainly algae, in a given volume of water in unit time; a procedure which has been aided greatly by the availability of the radio-active isotope of carbon, ^{14}C. However, the classical alternative method, introduced over half a century ago, of measuring the other part of the process of photosynthesis, the evolution of oxygen, is still in use and despite the fact that the ^{14}C method permits the estimation of much smaller amount of photosynthesis, the oxygen method is still not without certain practical advantages. This latter method was used by Prof. Pearsall and his associates to determine rates of photosynthesis in cultures of algae suspended at various levels in Windermere as early as 1933. If the nutrient supply is ample, then the rate of production is determined by the energy supply which, in the words of a famous advertisement, must be not too much and not too little. Thus we need measurements of light and temperature over the depths through which photosynthesis experiments are carried out. Methods were developed early in the Association's history. Prof. Pearsall and Mr., now Prof., Ullyott utilised new methods of measuring light and Mr. Saunders of temperature during the first few years after the Windermere laboratory opened. Later on Dr. Mortimer brought into use recently developed methods. To all of them the Association owes much in many ways and they may well be pleased that the recently appointed plant physiologist, Dr. J. F. Talling, is concentrating on the study of primary production. Photosynthesis is the basis of the growth of the algae we are discussing today so that it is clearly important to determine how close the relationship between the two is. We may expect that the simpler the structure of the plant the closer the arrangement will be, because the steps between photosynthesis and growth will be fewer than, for example, in a complex organism like a flowering plant. Therefore growth is also now being followed, using much the same technique as for photosynthesis.

Little is yet known about biological factors. A serious gap in our knowledge is that we do not know to what extent animals graze on algae. We do know a good deal about the effects of parasitic fungi, an important aspect of algal ecology, which has been pioneered by the Association. It seems that the larger plankton algae are more or less immune from grazing, an astonishing state of affairs when one considers the situation in the sea. The smallest algae, the so-called nannoplankton have been little studied, except taxonomically, but we are now beginning to fill this gap in the picture. They may well be the main source of food for planktonic animals, and indeed young stages of other animals, although preliminary observations have been rather contradictory. One of the founders of the Association was the late Mr. D. J. Scourfield, a very distinguished amateur biologist, who had advocated founding a station for freshwater biology long before the Glasgow meeting of the British Association; indeed he had gone to Austria in 1907 for the

opening of the now famous laboratory at Lunz. One of his special interests was the nannoplankton and it is sad that he is not with us today, for he always hoped to see the Association carry out intensive work on these algae.

Some of the earliest work of the Association consisted of synchronous observations on the seasonal variations in the amounts of essential plant nutrients dissolved in the water and the abundance of the commoner algae, a subject to which Prof. Pearsall contributed much. Regular estimations at weekly or fortnightly observations have been made for over a quarter of a century. The value of a long series of observations is large, for it often provides clues as to which environmental factors are varying regularly in relation to algal periodicity, not by chance but because there is a causal connection between them. Some observations on diatoms go back not tens but thousands of years. Thanks to the ingenuity of the late Mr. B. M. Jenkin it has been possible to obtain cores from the lake deposits taking us back in time to the end of the ice age. The resistant nature of the silica composing the cell walls of diatoms enables us to follow their deposition on the bottoms of lakes. Recently Mr. F. J. H. Mackereth has made a simpler and very efficient corer so that today we can obtain enough cores in a week to occupy a diatomist for years. A happy but slightly embarrassing advance for both him and the pollen analyst. The results of these studies, preliminary though they are, are of great fascination. For example we now know that at or just after the close of the ice age all the lakes in the Lake District had an extremely rich flora of diatoms whose constitution suggests waters much richer in calcium than today's near distilled water. Indeed, we can safely say that in this period the Lake District had calcareous soil so that there must have been a strikingly different land flora irrespective of the temperature. Another great change is seen in recent times, roughly in the last hundred years, when again the diatom assemblage in the lake deposits shows a sudden change. This can be related to the mild pollution from the opening up of the district as a tourist centre and the change in the method of sanitation. I believe the latter change has had more influence than the former.

In this historical review I have skated in a rapid and generalised manner over various aspects of our work. I have had to ignore the applications which are mainly in the fields of water supply, disposal of industrial wastes, fisheries, particularly fish ponds, and biochemistry. However, as Prof. Buxton said long ago when the possible activities of the Association were being debated, 'it is by pursuing the fundamental things that we shall more readily, in the end, reach our practical and immediate objective'. Fundamental research has always been the main function of our Association though, naturally, we are happy to be able to help in solving 'practical and immediate' problems.

I would like to conclude by saying something about the future, though prophecy in this sphere is often almost as foolish as astrology. We know relatively little about chemical factors and I believe that the greatest need for the future is more concentration on chemistry. Two of our difficulties are our relative ignorance about certain trace elements and almost all the organic compounds. A great landmark in the Association's history was the appointment in 1947 of a chemist, Mr. F. J. H. Mackereth, who is still with us. In my view we need to enlarge the chemical section because one man and his assistant are insufficient to cover this great field. All our culture work, which involves chemistry, will lose much of its value if it cannot be related to the varying chemistry of the natural environment. Indeed it is extraordinarily difficult to devise cultural procedures which will give answers which, from observation, we have not good reason to suspect are inapplicable to natural conditions.

In a diagram in a recent paper (Lund 1957, fig. 3) I compared certain plant nutrients in the water relative to those incorporated in a subsequent crop of plankton algae in Windermere. The crop was virtually unispecific, a fortunate

case not very uncommon in Windermere. This is but one more of the great advantages given to us by the wisdom of the Council in insisting on a laboratory on its shores against considerable opposition and at a time when the Association badly needed support from all quarters. It seems that only one of the elements for which analyses were made is likely to limit the size of the crop. Yet, though we know that this element, silicon, is important in determining the periodicity of this and other diatoms in Windermere, we also know that in other lakes other unknown substances control their abundance. Moreover we cannot produce such a budget as this for any other alga in the English Lake District or, I believe, elsewhere. Some further idea of our ignorance can be gained by examining this diagram which also shows the maximum size of the crops in some of the major lakes in the district. There are two groups, one poor and the other rich in algae. The latter are vastly richer than would be expected from the results of our chemical analyses. This suggests that we do not know what are the controlling nutrients. The need for the future is two-fold. First to continue and enlarge experiments with laboratory cultures. Here we are getting valuable help from the results of work done in universities and marine biological stations. Our own main aim, therefore, should be to concentrate on the second need, namely to devise methods for experiments with natural populations in natural water in the lakes concerned. If this can be done then rapid progress may be made.

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EXPERIMENTS WITH POPULATIONS OF FISH IN WINDERMERE

By E. D. Le Cren

It falls to me, in the last paper in this series, to attempt to describe some of the zoological work done by the Freshwater Biological Association, work on the ecology of fish. For obvious reasons fish are important among freshwater organisms and have been subjects for research at Wray Castle and The Ferry House almost from the start of the laboratories.

In the limited space available I am going to concentrate on one particular piece of research on fish ecology and try to describe its progress so far; rather than provide an historical review of all the fish work done by the Freshwater Biological Association. In concentrating on populations of fish I do not wish you to forget that research on fish has many links with other aspects of freshwater biology. The fish depend for their food on food chains that start with algae, and are greatly affected, both directly and indirectly, by physical factors such as the temperature of the water.

Windermere contains ten species of fish, of which seven—the trout, char, pike, perch, eel, minnow and stickleback are of ecological importance. Work done by my predecessors and colleagues, especially Mr. Allen, Dr. Frost and Mr. Smyly, has established the basic natural history of these species and revealed their food relationships. The char, young perch and to some extent the trout feed on zooplankton; trout, medium sized perch, eels, minnows and sticklebacks eat bottom fauna such as snails, shrimps and insect larvae; the pike, larger perch and larger trout feed on the fish (including their own species) (Frost, 1946).

Such a community of fish is typical of many lakes and lochs in northern Europe; usually the perch is the most abundant fish and the pike the chief fish predator. In 1940, Dr. Worthington, then Director at Wray Castle, started an experimental

fishery on Windermere. He had three main objects in mind; firstly, he wished to find out if significant amounts of freshwater fish could be caught as food in wartime; secondly, he hoped that a reduction in the numbers of perch and pike would improve angling for the more valuable trout and char; and thirdly he wanted to carry out a fairly large-scale ecological experiment (Worthington, 1949). As a result of the fishery some 80 tons of perch and 10 tons of pike and eels were sold as food, and the angling for trout and char probably improved. These aspects need not, however, concern us any more; I propose now to concentrate entirely on the effects of the fishery on the populations of perch and pike themselves.

The perch were caught in unbaited traps set in shallow water each spring during the spawning season, and were trapped intensively from 1941 to 1947 inclusive. Since 1948, however, only sampling has been carried out in the North Basin of Windermere, though removal of perch has been continued each year in part of the South Basin. I shall be concerned only with the North Basin, which can

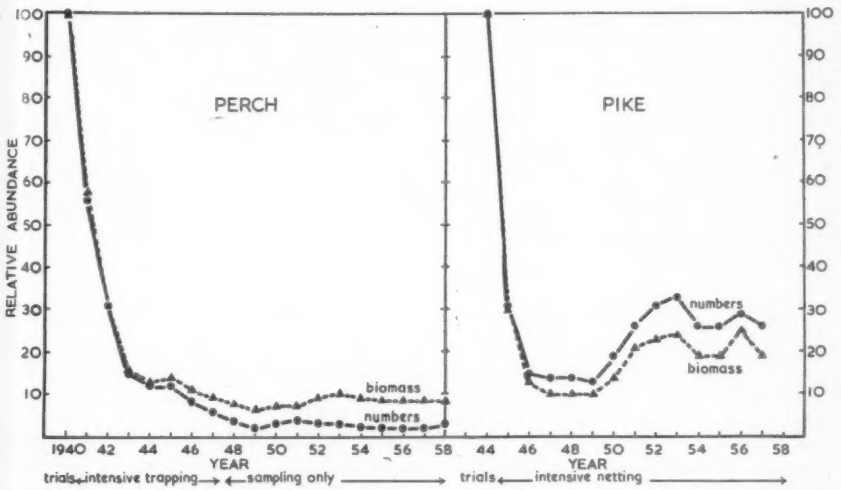

Fig. 1.—Changes in the abundance of perch and pike in Windermere with intensive fisheries.

be considered a separate lake. Since 1944 the pike have been caught in gill nets set all round the lake during the winter months (when there is no tourist boating).

I will deal first with the changes in the population of perch. Between two and three million perch were caught in the first few seasons' trapping but the catches declined rapidly (Fig. 1). Sampling by other methods confirmed that this was a real fall in population density and that the catches per trap give a good idea of the relative density of adult perch. It will be seen that by 1948 the numbers of perch were but 3 per cent of their original density and that they have remained at about this level since then in spite of the fact that the trapping was discontinued. The biomass, or total weight, of perch has stabilised at a rather higher level of about 8 per cent of the 1940 density.

It was thought in 1940 that the small average size of the perch was due to stunting caused by overcrowding, and that one result of thinning the population would be an increase in growth-rate and size. At first there was little significant increase in size, but recently it has been quite marked (Fig. 2). Some of the fluctuations in average size are due to the influx of large broods of young, etc., but most of the increase in size is due to an increase in growth rate. Perch hatched in

1952 and other recent years are now growing up to about eight times as fast as the adult perch grew before 1940 (Le Cren, 1958).

The catches of pike show a similar but even more rapid reduction in population density (see Fig. 1). At first the density appeared to stabilise at about 10–12 per cent of its pre-1944 level, but a temporary increase in the numbers of young pike has caused the catches in recent years to rise to about 25 per cent of their original level, though some decline may be expected in the next year or two.

The general level of a stock of fish such as the perch or pike depends primarily on the intensity of mortality, both natural and fishing. If survival of the fish from year to year is poor, a small population of fish that are mostly young will result. If, on the other hand, survival is higher there will be an accumulation of older fish

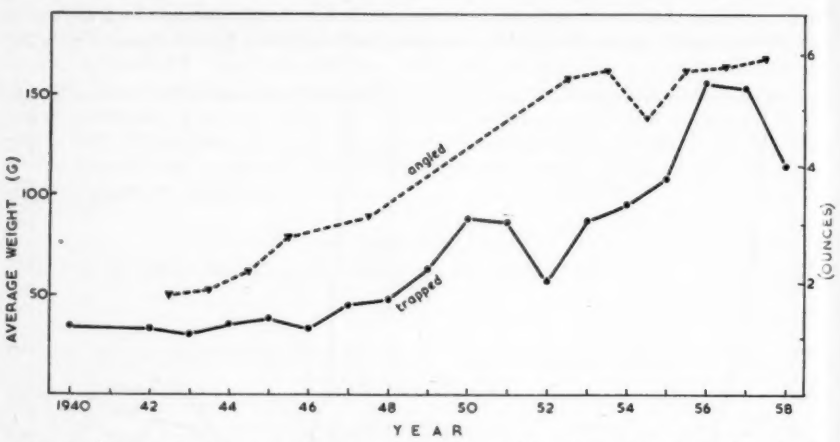

Fig. 2.—Changes in the average size of trapped and angled female perch in the north basin of Windermere.

and a bigger stock will result. The immediate and rapid declines in population densities of perch and pike after the start of the fisheries were due to increased mortality directly caused by the fisheries themselves. In the long run, however, populations are also affected by the numbers of young fish that are being recruited to the stock. It is of interest therefore to examine briefly the changes in the numbers of young perch and pike.

Taking the perch first, Fig. 3 shows the numbers of three-year-old perch caught each year plotted against the year of their hatch. It will be seen that the recruitment of three-year-old perch is very variable. The brood strength declined during 1941–5, was very low in 1946 and 1948 and has fluctuated since then with stronger broods hatching in 1949 and 1955. When this graph of young fish is compared with the one representing eggs, it will be seen that there is little correlation. The size of a brood depends much more on its survival than on the number of eggs that gives rise to it. There is evidence from synchronous variations in other lakes to show that the extreme fluctuations in survival are due, at least indirectly, to climate variations and are independent of population changes, natural or artificial (Le Cren, 1955). Apart from these year-to-year fluctuations, however, there has been a general lowering of the average survival since 1945 to about 60 per cent of what it was from 1941–5.

Exactly comparable data to these on young perch are not yet available for young pike. Some idea of the changes in the size of pike broods can, however, be obtained from the catches of small pike, mostly two-year-olds, that are made incidentally

in the perch traps (Fig. 3). The picture presented by these catches is quite different from that of the perch. Before the brood that hatched in 1946, the numbers of young pike fluctuated about a relatively low level; then, the broods were much more abundant for three or four years, particularly those of 1947 and 1949, but eventually declined again, till recent broods have had a similar strength to those before 1946. The numbers of eggs fell steeply after the start of the fishery in 1944, so the survival of young pike must have increased greatly. In recent years the number of eggs laid has been about 25 per cent of that before 1944, the numbers of resultant two-year-old pike very similar, and the survival in early life, therefore, about four times as great.

It is probable that predation from young pike and larger perch is one of the main natural causes of mortality among young perch. As the increased growth and

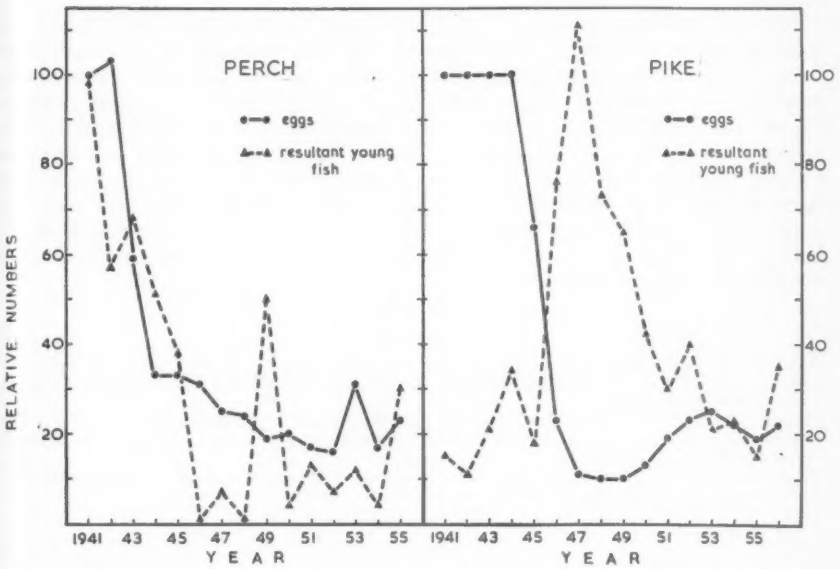

Fig. 3.—Changes in the numbers of perch and pike eggs laid each year (estimated from the abundance and size of females) and the numbers of fish resulting at the age of two to three years. (The scales of the graphs are relative; no attempt has been made to indicate the actual survival from egg to young fish.)

size of the perch has led to there being more perch of a fish-eating size now than in 1940 (in spite of the much smaller total number of perch), and as the numbers of young pike are at least as great as in 1940, the smaller number of perch that hatch must have to suffer a greater predation pressure. It is perhaps not surprising, then, that their survival in early life averages only 60 per cent of what it used to do, and that they have not, therefore, increased in numbers since the end of the trapping in 1948.

Finally, what conclusions can we draw from the results obtained from this experiment so far ? Firstly, it is clear that the numbers of fish of each species, even in a simple fish community such as that in Windermere, are governed by a number of complex and inter-related factors. In the background, all the time, are those climatic variables that, working both directly or indirectly through the food chain, can cause large year-to-year fluctuations in growth or the survival of broods of young fish. In Windermere, I have shown, for example, that the growth of perch

can be expected to be one third greater or less than average one year out of every three; and the survival of young perch can vary one-hundred-fold from one year to the next (Le Cren, 1955, 1958).

On top of these fluctuations, and often obscured by them, are those induced by the predator prey relationships among the fish themselves or by our experimental fishing. We have shown that it need not require much fishing effort to reduce, in two or three years, a stock of fish to a small fraction of its former abundance. The perch have in the long-run responded to this with a great increase in growth rate; this in its turn has increased their average fecundity and profoundly altered the balance of the different kinds of food eaten by the perch. The reduction in the numbers of larger pike has resulted in an increase in the survival of the young so recruitment has not been decreased. Other effects may well follow in due course or be revealed as detailed analysis is completed; we hope the experiment will be continued for some years yet.

I believe that this experiment is the kind of work which is particularly suited to a research station such as the Freshwater Biological Association. It essentially rests on an accumulation of basic information about the fish and their environment collected by a number of people over a long period of time. It is an experiment on a scale sufficiently large to provide adequate numerical data, yet small enough to be within the bounds of practicality so that the operations involved can be done entirely under the control of the experimentors and not involve outside commercial interests. This type of work occupies what I believe to be an essential mid-way position between for example the university experimentalist who works with ten fish in a controlled tank, and the marine fishery biologist who works with the catch statistics of a fleet of deep-sea trawlers.

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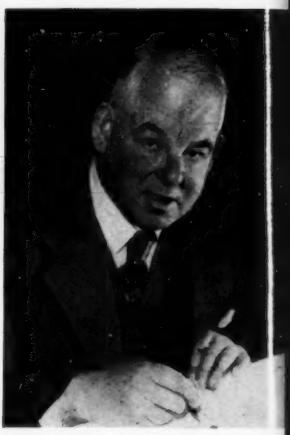

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